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Research Report

Effects of Military Service on Earnings and Education, Revisited

Variation by Service Duration, Occupation, and Civilian Unemployment

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RAND National Security Research Division

Prepared for the Office of the Secretary of Defense

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Preface

Policymakers and the public at large have expressed concern about the economic well-being of veterans returning home from service overseas; these concerns have grown in recent years during the economic recession.

The research described in this report builds on research conducted by RAND Corporation researchers in 2011 with support from the U.S. Army (Loughran, Martorell, Miller, and Klerman, 2011). They estimated that, for as many as 18 years following enlistment, the causal effect of military service on labor market earnings and on a critical determinant of earnings, educational outcomes. They addressed the empirical problems associated with the selective nature of military service by restricting their analysis to military applicants, some of whom enlist and some of whom do not, and by controlling for a rich array of applicant characteristics available on the military application record. They found that military enlistment increases earnings in both the short and long terms, although there is a large dip in earnings in the period immediately following separation from military service. Enlistment delays college education and decreases the likelihood of obtaining a four-year college degree but increases the likelihood of obtaining a two-year degree.

This report addresses some possible driving forces behind those returns, outside factors that could also affect them, and the potential for the military to directly affect postmilitary earnings. The goal of this research, supported by the Office of the Secretary of Defense, was to extend the work, honing in on the earnings dip and disaggregating the data to look at differences in earnings and education depending on a service member's length of service, military occupational specialty while in the service, and labor market conditions present at the time of separation from military service. We also analyzed the effect on earnings of an Army recruiting program called Partnership for Youth Success.

This research will be of interest to the U.S. Department of Defense, the U.S. Department of Veterans Affairs, and policymakers who have responsibility for recruiting, setting appropriate compensation, and developing programs that help veterans make the transition from military to civilian life. It will also be of interest to researchers interested in the return to military service and the experiences of service members who transition to civilian life.

This research was sponsored by the Office of Accession Policy within the Office of the Under Secretary of Defense for Personnel and Readiness and conducted within the Forces and Resources Policy Center of the RAND National Defense Research Institute, a federally funded research and development center sponsored by the Office of the Secretary of Defense, the Joint Staff, the Unified Combatant Commands, the Navy, the Marine Corps, the defense agencies, and the defense Intelligence Community.

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Summary

The overriding objective of U.S. military compensation policy is to attract and retain the force necessary to meet the nation's national security objectives. Whether and how military service affects earnings and an individual's likelihood of completing college—one determinant of future earnings—has implications for military policies related to compensation, recruiting, and retention. For instance, if an individual contemplating military service can be shown evidence that he or she will be well-served by the experience, it could make that person more willing to enlist. On the other hand, if there are economic hardships associated with military service, policymakers responsible for maintaining the force could use information on why this is occurring to help formulate policies to mitigate these hardships.

Estimating the effect of military service is complicated by the fact that veterans are likely to be different from nonveterans in ways that are correlated with subsequent economic outcomes but are not observable to the researcher. In a 2011 study (Loughran, Martorell, Miller, and Klerman, 2011 [hereafter LMMK]), RAND researchers overcame this econometric problem by following the approach pioneered by Angrist (1998), in which applicants to the military who enlisted were compared with military-eligible applicants who chose not to enlist, controlling for the detailed covariates available on the military application record. LMMK examined the effects of military service on earnings and educational attainment for all U.S. Army, Navy, Marine Corps, and Air Force enlisted personnel and found that enlistment slightly increases the likelihood of pursuing higher education and increases earnings but with a substantial earnings dip in the period immediately following separation.

This report builds on the earlier work in an effort to understand the ways in which military service affects earnings. We were especially interested in understanding how these effects differ by how *long* service members served (years of service, or YOS) and their military occupational specialties (MOS) while serving. We also sought to understand how external factors and policies affect these impacts so as to potentially increase the returns to military service. To do this, we examined how economic conditions in the civilian labor market when individuals exit active duty affect postservice earnings, and we studied the effect on earnings of an Army recruiting program designed to promote enlistment but with the potential to ease the financial transition from military to civilian life.

The Data

This study employed an array of administrative data sets. The Defense Manpower Data Center (DMDC) Proxy PERSTEMPO file was our main data source on length of service, MOS, timing of separation, initial contract length, and reenlistment decisions.

The Military Entrance Processing Command (MEPCOM) has records for every individual who applies for enlistment into the active-component military service. The records include demographic information (race/ethnicity, gender, and age); the applicant's home state; educational status; his or her percentile score on the Armed Forces Qualification Test (AFQT); and results from a physical examination, background check (to look for contact with the criminal justice system), and drug and alcohol tests.

We constructed annual earnings measures from administrative data sources, including the Social Security Administration Master Earnings File (MEF) and DMDC Active and Reserve Military Pay Files. The MEF covers nearly all sources of wage income in the United States. We had MEF records for the period 1985–2010. For all calculations, total earnings were converted to 2005 dollars using the Consumer Price Index.

An important omission from the MEF, from the point of view of this project, is forms of military pay that are not subject to Medicare tax. In particular, certain military allowances (e.g., basic allowance for subsistence [BAS], basic allowance for housing [BAH], Family Separation Allowance [FSA]) and bonuses are not subject to Medicare taxes. However, these payments are included in the military pay files. The military pay files are first available for 1994. With this information, we created a total earnings measure equal to the sum of earnings on the MEF and military income.

In addition, education data from the National Student Clearinghouse allowed us to track military applicants as they transition in and out of college and complete college degrees. We supplemented these data with information on Montgomery GI Bill (Pub. L. 78-346, 1944) usage for analyses for which we did not have Clearinghouse data linked to personnel records. Because almost all veterans who have served for at least three years are eligible for tuition assistance, we used data on utilization of the Montgomery GI Bill (either the active- or reserve-component programs) as a proxy for going to college. The data files that contain information on utilization include the date on which the veteran began receiving benefits, the number of months that benefits were paid, and the total amount of the benefit payments.

Although the analyses presented in this report are unified in that they all address questions related to the economic well-being of veterans, the samples used for each analysis differ. For the investigation of the effect of military service on earnings, we focused on individuals who applied to the military in 1994 because this is the earliest cohort for which we have complete data on earnings and thus allows the longest follow-up. For reasons discussed in Chapter Four, the analysis of the effect of military service on educational outcomes, we focused on the 1991–1994 cohorts of military applicants for degree outcomes and the 1998–2000 cohorts of applicants for enrollment outcomes. For the analysis of the effect of economic conditions at separation, we cast a wider net to ensure sufficient variation in economic conditions at the time of separation, focusing our analysis on the 1991–2006 entering cohorts. Finally, to examine the impact of the Partnership for Youth Success (PaYS) program, we looked at all first-time Army enlistees between January 2001 and October 2004 because these cohorts spanned the time frame in which

PaYS was introduced and significantly expanded. Note that all analyses are limited to enlisted personnel; officers are not included in any of the results reported here.

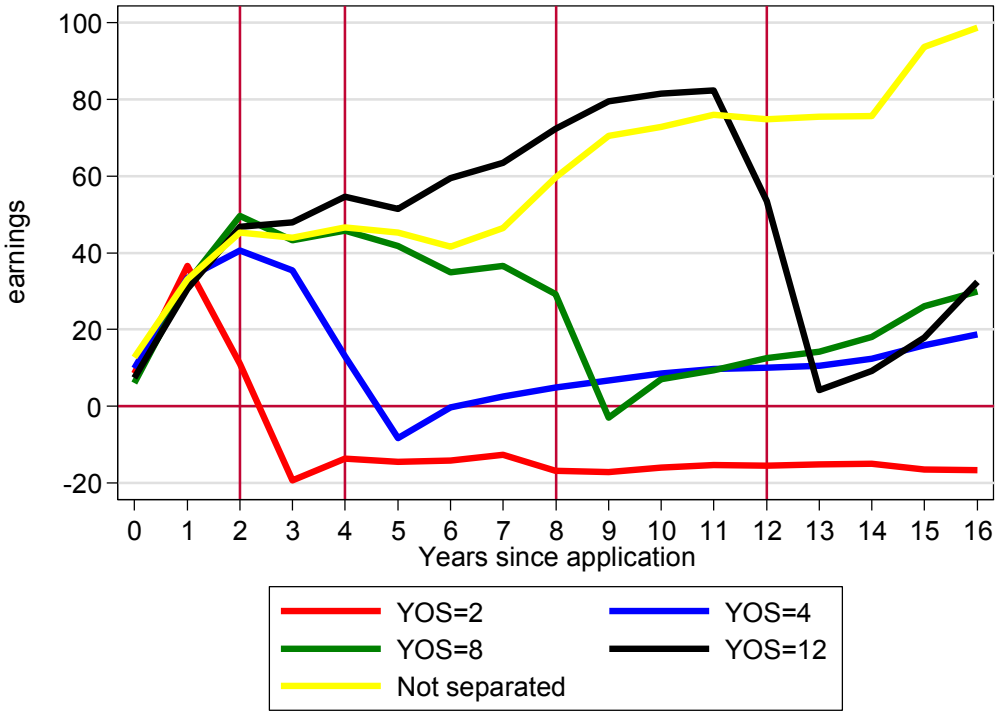
Analyses of Earnings and Education, by Years of Service

We began our analysis of the effects of military service on earnings by examining how these vary with YOS because the findings in LMMK suggested that there was a "dip" in the effects around the time that many individuals would be separating from the military. By examining results by YOS, we could assess whether this dip coincided with the exact timing of separation. One common feature across all YOS values (for the individuals who actually separate from the military) is that earnings fall sharply in the year of separation. This phenomenon could be due to the compensating wage differential paid to service members while they are in the service followed by the abrupt end of full-time employment. Many begin college in the same year they separate or compete for civilian jobs against nonveterans who have more civilian work experience.

We looked at YOS because there are likely to be differences in earnings declines and college enrollment by the timing of separation. Premature separation (e.g., in years 1 to 2, before the end of the first contract) may be a signal of a "poor-quality" recruit. Thus, we expected relatively high rates of college enrollment along with steep earnings declines immediately following service, but the lifetime impact is unclear.

By the same token, service members separating after two or more terms may have characteristics that would make them more likely to have higher incomes upon separation. The lifetime earnings pattern for service members with one versus two terms of service is unclear. Figure S.1 illustrates the estimated effect of enlistment on total earnings (i.e., civilian plus military, including for reserve pay) measured as a percentage of mean earnings in a particular year for individuals in the sample by year since application to the military for those with two, four, eight, and 12 YOS.

Figure S.1
Estimated Effect of Enlistment on Annual Earnings (as a Percentage of Mean Earnings), by Years Since Application and Years of Service Prior to Separation, 1994 Application Cohort

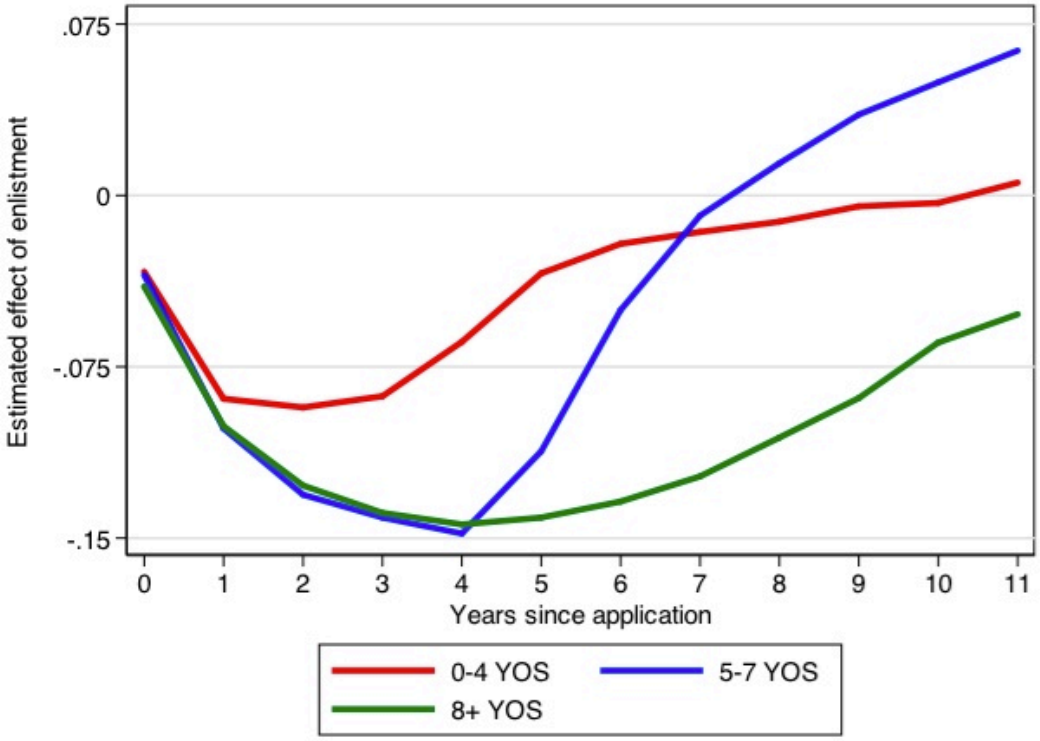


In LMMK, we found that military service delays college but that veterans enroll in and complete college at higher rates than comparable nonveterans. Using an econometric strategy similar to that used to examine the effects of military enlistment on earnings, LMMK evaluated the differences in educational attainment between veterans and comparable nonveterans stratified by YOS prior to separation.[1] These effects are shown in Figure S.2, which plots the estimated effect on the probability of college enrollment by year since application to the military and by YOS. We find that service members who serve five to seven years are the most likely to use their education benefits and do so soon after separation. Relative to those who separate after eight or more years, these service members are younger and less likely to be married, and they have less military experience that can substitute for education in the labor market. In contrast, service members who separate after eight or more years may have better job prospects at the time of separation, or college may be less desirable because of the need to cover family expenses. Although it is tempting to say that these service members use the military as an alternative to college, our findings suggest that many of them do eventually enroll in college. Service members

[1] For this report, *educational attainment* means having ever enrolled in a two-year or four-year college and completing a degree.

who separate with fewer than four YOS delay their college enrollment significantly longer than those who separate with five to seven YOS, but they do eventually catch up with their civilian counterparts. We urge caution in interpreting the results presented in Figure S.2 because of concerns about differences in service members' characteristics that are correlated with the timing of separation from military service, which are discussed in Chapter Three.

Figure S.2
Estimated Effect of Enlistment on Cumulative College Enrollment Rate, by Years Since Application and Years of Service Prior to Separation, 1998–2000 Application Cohorts



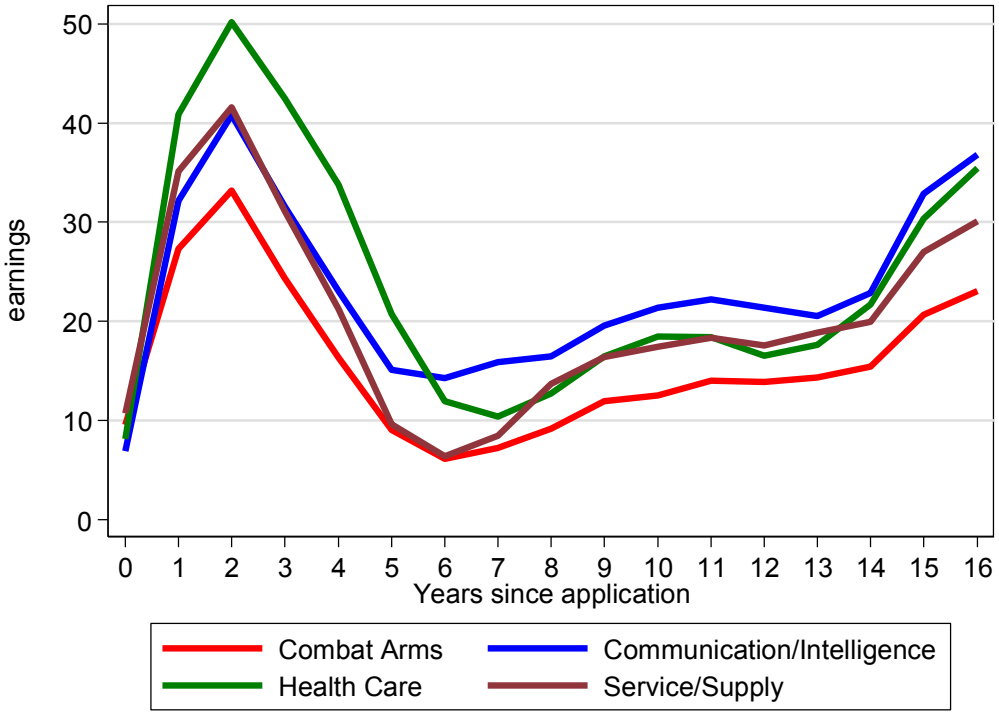
Analyses of Earnings and Education, by Military Occupational Specialty

We looked at MOS because the military may provide some benefits that are directly transferable to the civilian labor market. Service members entering military service with some MOS, particularly those that require specialized skills in such fields as health care that are also present in the civilian labor force, may not experience as dramatic a decline in earnings after separation as those entering other professions. This may be mediated by the need to supplement military training in these fields with formal education, particularly in fields that require credentials in the civilian labor market. We thus looked at how college enrollment varies by initial MOS as well. We find some evidence that the earnings trajectories are partially explained by these hypotheses.

For all occupations, the sizable earnings premium associated with military service in the first several years (see Figure S.3; effects measured as a percentage of mean earnings) declines over the next several years but remains positive, and then increases starting around year 7. Veterans who enter occupations in health care or communications or intelligence have larger gains (about 35 percent) over comparable nonveterans than do veterans who go into combat arms (about 23 percent). The earnings gains for veterans who go into service or supply occupations fall in between these two other groups.

Likewise, service members who enter the military in health care or communications are significantly more likely to enroll in and complete college and to do so earlier in life. Otherwise, there is very little difference in college enrollment and degree attainment patterns by MOS.

Figure S.3
Estimated Percentage Effect of Enlistment on Annual Earnings, by Years Since Application and Military Occupational Specialty, 1994 Application Cohort



Economic Conditions at the Time of Separation and Postservice Earnings

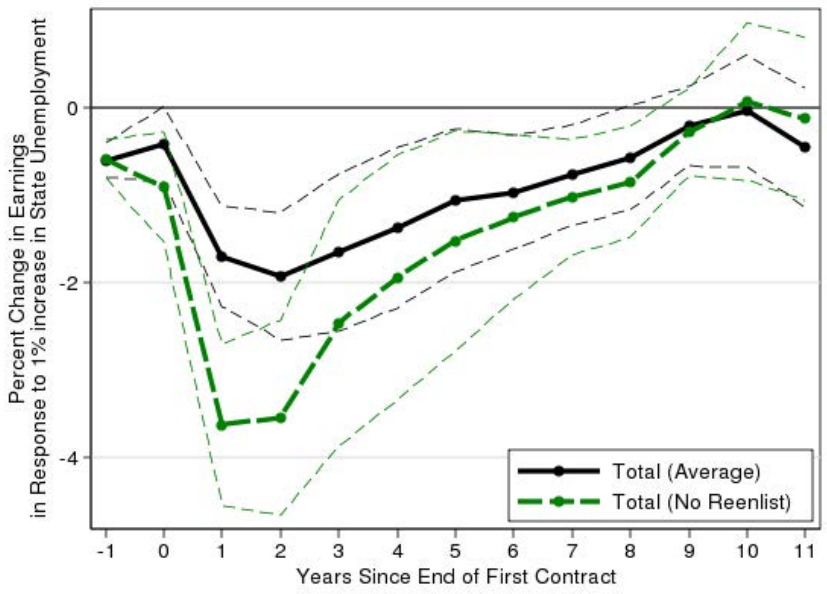
At enlistment, military service members commit themselves to an initial term of service, during which time there are predictable and unpredictable developments in the economy. We isolated the unpredictable component of economic conditions and examined the effect that unpredictable events and changes in economic conditions at the time when service members

separate can have on future earnings. We did so for enlisted service members who separated between 1993 and 2010. This sample gave us a large population of individuals subject to a similar economic environment, over a long enough time period to observe significant variation in state unemployment rates. We report the effect of changes in veterans' home-state unemployment rates on earnings, distinguishing between the effect on all service members and the effect on those who reenlist.

We find that both men and women are likely to reenlist and substitute military income for civilian income when civilian jobs are scarce. Despite this substitution, men's total earnings are lower in the period after separation when they separate in times of high unemployment. In contrast, we see little evidence of effects of economic conditions on the postservice earnings of women.

Figure S.4 shows the effects of home-state unemployment rates on total earnings (expressed as a percentage of average earnings) for those who do and do not reenlist after their first contracts. The "total" effect includes individuals who reenlisted, so the effect should be thought of as an average effect for those who separate and those who reenlist (who, at least for the period of their reenlistment, should not be affected by the unemployment rate). In contrast, the effect for those who separate applies to individuals who leave active duty. These effects have been adjusted for selection into reenlistment, though this has virtually no impact on the estimates. Whether or not they reenlist, men lose approximately 1 percent of earnings for each 1-percent increase in the unemployment rate. As expected, these losses are concentrated among enlisted men who do not reenlist before the end of their first terms. Peak losses for those who do not reenlist occur in the first and second years following separation and approach 4 percent of total earnings for each 1-percent increase in unemployment in these years. As striking as the nearly 4-percent peak total earnings losses are among those who do not reenlist, the total earnings losses here are not as steep as the decline in civilian income in these years, suggesting that those who do not reenlist continue to draw some military income.

Figure S.4
Estimated Effect of Home-State Unemployment on Annual Earnings, by Years Since End of Initial Contract, Men, 1991–2006 Entry Cohorts, by Reenlistment



Participation in the Army's Partnership for Youth Success Program and Earnings

The preceding evidence suggests that military veterans, on average, tend to earn more than their nonveteran counterparts but that difficult economic times at the time of separation do impose an earnings loss on veterans. This is especially relevant given the weak economic conditions veterans currently face. Given these results, we looked at other ways in which the military can ease transitions to the civilian labor market. We looked explicitly at the Army PaYS program. PaYS is a partnership between the U.S. Army and U.S. employers that allows the Army to offer postservice employment opportunities as a recruitment incentive. Each PaYS employer is expected to project future openings for separating service members by MOS and list those openings in an Army database. Participating employers commit to interviewing PaYS participants for those positions, but they are not obligated to hire them.

We evaluated the impact of individual participation in the PaYS program on subsequent annual military and civilian earnings among Army enlistees with four-year contracts. Our analysis looked at all first-time enlistees between January 2001 and October 2004. This allowed us to capture variation in PaYS take-up as the program was scaled up significantly in April 2003. Because analysis required individuals to be followed for at least five years, we were limited to enlistees before November 2004. To isolate the impact of the PaYS program from differences between participants and nonparticipants, we use ordinary-least-squares (OLS) regression

analysis to control for characteristics that are associated with earnings and potentially related to selection into the PaYS program, including aptitude, demographic characteristics, and date of enlistment. Our findings indicate that PaYS participation is not associated with an increase in earnings and, in fact, is associated with a slight decrease in earnings and therefore may not be effective at easing the transition from military to civilian life. However, we cannot rule out the possibility that these negative estimates are due to differences between participants and nonparticipants for which we were unable to control in our OLS analysis.

In interpreting the findings, it is important to note that the PaYS program is more an Army recruitment tool than a means for increasing earnings. Although the program can offer tangible benefits to service members, including the prospect of a job that is aligned with the skills they develop in the military, several factors need to occur for PaYS to result in increased earnings. There are many points at which the process might break down, resulting in no improvement in employment outcomes. Anecdotally, the Army reports that individuals are following through with interviews and, in some cases, receiving the job with the PaYS partner employer. It may be that these cases do not represent the majority or that the opportunities individuals receive with PaYS employers are no better than what they would have obtained without PaYS. Other military programs aimed at smoothing the transition to civilian life (e.g., the U.S. Department of Defense's [DoD's] Transition Assistance Program [TAP]) may be more effective, and they merit further research.

Policy Implications and Next Steps

Because of limitations in the analytic approach described in more detail in the body of this report, we view our estimates on the effect of military service on education and earnings by YOS and MOS, as well as the estimated impacts of the PaYS program, as suggestive rather than causal. Nonetheless, the analyses provide valuable information to DoD and policymakers regarding the impact of military service on the economic well-being of service members. This information is critical to maintaining a compensation system that offers competitive, attractive pay and benefits and developing programs that assist veterans in their transition to civilian employment without a prohibitive drop in earnings. The following are suggestions for next steps DoD and policymakers might consider:

- Develop credentialing programs that veterans can use to signal to employers that they possess certain skills. The findings described in Chapter Three of this report suggest that the "returns" to military service do not vary strongly with military occupation, although the degree to which skills are transferable almost surely vary by occupation. For instance, veterans who learn lots of skills in, say, truck repair or communication technology, might have skills that are very valuable to civilian employers, but these employers might not understand the military occupation labels. A credentialing program would help with this problem.

- Conduct more research on the effect of recent changes to veterans' educational benefits. The findings in this report focus on an era when veterans were eligible for the earlier GI Bills, but the passage of the much more generous Post-9/11 GI Bill (Pub. L. 110-252, 2008, Title V) means that military service might have different effects on educational attainment and earnings in the future. More work needs to be done on this question.
- Research suggests large differences in earnings by college quality. Consider providing information to service members to help them select and apply for college and otherwise optimize the use of their education benefits.
- Conduct more analysis of the effectiveness of veterans' transition-assistance programs. The U.S. Department of Labor and other government agencies have transition assistance programs, including TAP and the Work Opportunity Tax Credit (WOTC), but these have not been subject to rigorous evaluation. A better understanding of these programs is important, especially in light of the lack of evidence of beneficial effects of PaYS and the finding that veterans who exit in times of high unemployment have lower earnings.
- Determine how business-cycle fluctuations affect the demand for transition-assistance services. The research described in Chapter Six suggests that high unemployment at the time of separation lowers earnings and hence might increase the need for and the total spending on services for veterans entering the labor market (although this could be offset by increases in reenlistment, which would lessen the need for such assistance). This information is crucial for budgetary planning, and it will help DoD prioritize resources devoted to assisting service members in a successful transition to civilian employment. This will become increasingly important as reductions in end strength increase the fraction of service members transitioning into civilian employment.

Abbreviations

AFQT	Armed Forces Qualification Test
ATE	average treatment effect
BAH	basic allowance for housing
BAS	basic allowance for subsistence
DMDC	Defense Manpower Data Center
DoD	U.S. Department of Defense
FICA	Federal Insurance Contributions Act
FSA	Family Separation Allowance
ITT	intention to treat
LMMK	Loughran, Martorell, Miller, and Klerman, 2011
MEF	Master Earnings File
MEPCOM	Military Entrance Processing Command
MEPS	Military Enlistment Processing Station
MGIB	Montgomery GI Bill
MOA	memorandum of agreement
MOS	military occupational specialty
OLS	ordinary least squares
PaYS	Partnership for Youth Success
ROTC	Reserve Officer Training Corps
SSA	Social Security Administration
TAP	Transition Assistance Program
VA	U.S. Department of Veterans Affairs
WOTC	Work Opportunity Tax Credit
YOS	year of service

Chapter One. Introduction

In recent years, policymakers and the public at large have expressed concern about the economic well-being of veterans returning home from service overseas. In particular, there is widespread concern that the slow recovery from the economic recession is hitting veterans especially hard (Beucke, 2011). Although some recent research has shown that veterans may be faring no worse than other young people (Heaton and Krull, 2012), the performance of veterans in the labor market remains an issue at the forefront of the national policy discussion.

The perception that veterans are doing especially poorly in terms of employment and earnings is particularly noteworthy because the military has long marketed itself as offering economic opportunities for individuals, especially those from disadvantaged backgrounds. Military service could have beneficial effects on labor market performance for a variety of reasons. Most obviously, it provides a full-time job with relatively high wages (Office of the Under Secretary of Defense for Personnel and Readiness, 2008a, 2008b). It could also provide service members with work experience that could carry over to civilian jobs and could teach "soft skills," such as discipline and punctuality (Furstenberg, Rumbaut, and Settersten, 2005). The military also offers generous educational benefits in the form of the Montgomery GI Bill (MGIB), Army College Fund, and other programs that provide tuition assistance for postsecondary schooling. Crucially, any effects on earnings could occur not only because of what happens during service but also because going through military service changes decisions about human capital investments (e.g., schooling and training).

Whether and how military service affects earnings is an important policy question. For instance, it has implications for compensation policy. The extent to which enlistees have higher or lower earnings as a consequence of entering the military is relevant for whether military pay is set at the appropriate level. The effects also have implications for accession policy. On one hand, if enlistment has economic benefits, then recruiters can market this information to potential enlistees considering entering the military. On the other hand, if there are economic hardships associated with military service, or that veterans in particular face, then policy could address the underlying reasons. For instance, it may be that, because the timing at which one separates from the military is at least somewhat out of a service member's control (in the sense that the service member is contractually obligated to serve some minimum amount of time), exiting the military during economic downturns, such as the current one, may have negative consequences for future earnings potential. If this is the case, then effective policies that help veterans transition into civilian employment might be important.

Several prior studies have examined the effect of enlistment on labor market earnings, starting with the pioneering work by Angrist (1998). In that study, Angrist (1998) compares the earnings of veterans with those of nonveterans who applied to the military but chose not to enlist.

The insight behind this approach is that, because all applicants demonstrate a serious interest in the military, an enlistee will tend to be more similar to applicants who did not enlist than to individuals who did not apply. Angrist (1998) further refined these comparisons by controlling for the rich set of applicant characteristics included on the military application record. Given that the application record contains the information used to screen recruits, it might be that these controls are sufficient for eliminating differences between applicants who do and do not enlist that are relevant for later-life earnings (although some differences based on unobservable factors might very well remain).

Although Angrist (1998) found little effect of military service on earnings for whites and only moderate effects for nonwhites, subsequent research (Loughran, Martorell, Miller, and Klerman, 2011 [henceforth LMMK]) showed that, when a more comprehensive measure of earnings was used (that included earnings paid by the military not subject to Federal Insurance Contributions Act [FICA] tax), there were substantial average gains of military service across a broad range of applicant subgroups. LMMK also examined the effect of military service on postsecondary schooling and found modest effects on whether veterans received college degrees (although these effects were too small to explain much of the earnings effects).

One issue that arose in LMMK is that, although service members generally experience an earnings premium relative to otherwise-similar nonveteran applicants for military service, there is a substantial dip in this earnings premium as service members separate from military service. The project described in this report builds on LMMK by attempting to better understand the ways in which military service affects earnings. After describing the data we use for the analyses in Chapter Two, we examine the extent to which the average effects of enlistment varied with the nature of an enlistee's military experience. In particular, we are especially interested in understanding how these effects differ by how *long* service members served (years of service, or YOS) and their military occupational specialties (MOS) while serving. Thus, in Chapters Three and Four, we address these questions by estimating effects of military enlistment on earnings and postsecondary schooling separately by length of time in the military and by MOS.

We also seek to understand how external factors and policies influence these effects because this knowledge might be useful for making the returns to military service larger. To do this, we examine how economic conditions in the civilian labor market that exist at the time individuals exit active duty affect postservice earnings. Relative to the cohorts studied in our analysis, service members exiting from service today face considerably worse economic conditions. To assess the impact of this change, in Chapter Five, we examine the extent to which economic conditions at the time of separation from the military affect long-run earnings.

Finally, recognizing the challenges faced by service members transitioning to civilian life, the military has developed programs aimed at smoothing this difficult transition. In Chapter Six, we examine the effect of participation in one such program, a recruiting program established by the Army called the Army Partnership for Youth Success (PaYS) program.

Chapter Two. Data Sources

In this chapter, we describe the data sources used in this project and the organizations that collect and provide the data. These details are relevant to understanding the information in the rest of this report related to service members' experiences in the service and after separation. The details are also relevant to the approach we took to the project because the content and scope of the data vary according to the goals of the organizations that collect them, and we adapted our approach, and added caveats to our findings, according to what the data could and could not tell us. Note that all analyses are limited to enlisted personnel; officers are not included in any of the results reported here. Because the different analyses discussed in this report differ in terms of the sample period, data requirements, and population of interest, we defer discussion of the details for each sample used in a given analysis to the individual chapter that covers it.

Military Entrance Processing Command

The information on applicants to the military that we used to study the relationship between enlistment and earnings (Chapter Three) and college attainment (Chapter Four) comes from the Military Entrance Processing Command (MEPCOM) administrative records. The MEPCOM data have records for each individual who applies for enlistment into the active-component military service. An individual interested in joining the military begins the application process by going to a Military Enlistment Processing Station (MEPS), completing an application, taking the Armed Forces Qualification Test (AFQT), and getting a physical exam.

For each applicant, the MEPCOM data include information used by the military to screen potential recruits. This includes educational status and results from the physical exam, a background check (to look for contact with the criminal justice system), drug and alcohol tests, and the AFQT (expressed as a percentile score). The AFQT in particular is a very important measure of cognitive ability and has been shown to be a powerful predictor of labor market earnings (Neal and Johnson, 1996). Other information contained on the application record includes basic demographic information (race/ethnicity, gender, and age), as well as the service component to which the applicant applied. The MEPCOM records also include an applicant's home state and county. We used this information to construct measures of local economic conditions at the time of separation from the military (Chapter Five).

Individuals who pass the military screenings are given the chance to enlist. As described in LMMK, accepted applicants receive an offer consisting of such elements as an enlistment bonus, tuition-assistance benefits, MOS (including training specific to the MOS), and initial term of service. The elements of the offers vary depending on the applicant's characteristics and military requirements at that moment. The applicant can select one of the offered options or choose not to

enlist. Those who choose one of the offered options sign a formal enlistment contract. When a recruit enlists, an accession record is generated and included in the MEPCOM records. As described below, we matched applicant and accession records to identify which applicants entered the military.

Proxy PERSTEMPO

The Defense Manpower Data Center (DMDC) Proxy PERSTEMPO (henceforth "PERSTEMPO") file was our main data source on length of service, MOS, timing of separation, initial contract length, and reenlistment decisions. As described in Asch, Heaton, et al. (2010), this file consists of monthly records of all active-duty service members. For each month a service member is on active duty, the file has information on characteristics, such as MOS, pay grade, duration and location of deployment overseas, and the time remaining on the current term of service.

One of the key uses of the PERSTEMPO data for this study was to determine an individual's initial contract length and whether and when an individual reenlists or separates from the military. The enlistment contract specifies the period of initial service. That contract length varies with service, over time, and across individuals. Typically, the initial contract is three to six years. At the end of the first enlistment contract, individuals choose whether to reenlist in the military or to separate and enter the civilian sector. This decision reflects both the service member's preferences and the military's decision concerning its need for the individual's continued service and the individual's suitability for continued service (see Asch, Heaton, et al., 2010, and Hosek and Martorell, 2009, for further details).[2] Because the PERSTEMPO file has the number of months left on a service member's current contract, we were able to separate out the length of the initial contract, as well as identify when an individual separated from the military or reenlisted.

Administrative Earnings

The analysis of earnings in Chapters Three and Five uses an annual earnings measure constructed from the Social Security Administration (SSA) Master Earnings File (MEF) and the DMDC Active and Reserve Military Pay Files. The MEF covers nearly all sources of wage income in the United States. We have MEF records for the period 1985–2010.[3]

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[2] Two other factors are important for a reenlistment decision, as noted in LMMK. The first is the retirement benefits, which are quite generous and are offered to individuals with 20 or more YOS. This creates strong incentives to remain in the military after an individual has already reenlisted once or twice. The second is success in the military, a marker of which is the speed with which one has been promoted (Hosek and Martorell, 2009).

[3] As noted in LMMK, we can match about 95 percent of the military application records to the MEF. Virtually all military applicants should appear in the SSA data because basic pay is subject to Medicare taxation. Similarly, almost all nonenlistees are employed in a covered job at least once between 1985 and 2010 (and hence should match

An important omission from the MEF for this project is forms of military pay that are not subject to Medicare tax. In particular, certain military allowances (e.g., basic allowance for subsistence [BAS], basic allowance for housing [BAH], Family Separation Allowance [FSA]) and bonuses are not subject to Medicare taxes. However, these payments are included in the military pay files. The military pay files are first available for 1994.

With this information, we created a total earnings measure equal to the sum of earnings on the MEF and military income. For the analysis in Chapter Three, we also included a measure of the tax advantage of military income. This is because the non-taxable income that military personnel receive is more valuable on an after-tax basis than is regular income that is subject to tax. These tax imputations assume that enlisted individuals file as single with no dependents.[4] For all calculations, total earnings were converted to 2005 dollars using the Consumer Price Index.

National Student Clearinghouse College Enrollment and Degree Records

Our education data come from the National Student Clearinghouse. Founded in 1993, the Clearinghouse is a nonprofit organization that contracts with institutions of higher education to verify college enrollment and degree receipt for student-loan agencies. The Clearinghouse data allow us to track military applicants as they transition in and out of college and complete college degrees. The Clearinghouse maintains college enrollment data for institutions in years in which those institutions had an active contract with the Clearinghouse. For reasons described in Chapter Four and LMMK, we matched the MEPCOM records from a stratified sample of 120,000 military applicants from the 1991–1994 cohorts to Clearinghouse degree-completion records to study the impact of military service on degree completion. We also matched MEPCOM records from a stratified sample of 120,000 military applicants from the 1998–2000 cohorts to Clearinghouse enrollment records to study the impact of military service on college enrollment. With these cohorts, we were able to measure 67 percent of all college enrollments for the 1998–2000 applicant cohorts and 63 percent of all awarded college degrees for the 1991–1994 applicant cohorts.

GI Bill Utilization

In the analysis of the effect of local economic conditions on earnings after separation, an important consideration is how local economic conditions affect the decision to go to college upon separation from the military. The military offers generous tuition-assistance benefits. Most enlistees who serve for at least two years are eligible for education benefits through the MGIB.

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to the MEF). However, the match rate will be less than 100 percent in practice because of discrepancies in the names, Social Security numbers, and dates of birth used to match the applicant and SSA records.

[4] See LMMK and Klerman, Loughran, and Martin (2005) for further details on this tax imputation.

In addition, because many service members enter the reserves after leaving active duty, they might also be eligible for benefits through the MGIB Selected Reserve program. Although the details of how the MGIB program has been administered have changed over time, the primary function of the program is to provide a tuition stipend for up to 36 months of educational expenses. Recent empirical research has shown that variation in the generosity of MGIB benefits over time is strongly associated with the college attainment of military veterans who were eligible for the benefit (Simon, Negrusa, and Warner, 2010).

We used data on utilization of the MGIB (either the active- or reserve-component programs) as a proxy for going to college in the analyses of Chapter Five that include only military personnel.[5] The data files that contain information on utilization include the start date of benefit receipt, the number of months that benefits were paid, and the total amount of benefit payments.

[5] Note that we cannot use these data in the analysis conducted in Chapter Four because they offer information on college enrollment only among veterans, but we need information on college-going of nonveterans for this analysis.

Chapter Three. Analysis of the Effect of Military Service on Earnings

We begin by looking at the earnings differences between veterans and comparable nonveterans stratified by YOS prior to separation and by first MOS. We looked at YOS primarily because it allows us to focus on what happens after separation because everyone in a given entry cohort with the same YOS will have separated in the same year. We also do so because there are likely to be differences in earnings declines and college enrollment by the timing of separation. We are also interested in understanding whether the earnings "dip" in the effects around the time that many individuals would be separating from the military coincides with the exact timing of separation. We looked at MOS because the military may provide some benefits that are directly transferable to the civilian labor market, in which case we would expect that service members in those occupations—health care and communications, for example—would experience a lower drop in earnings at separation. For those entering other professions, to prevent a dramatic earnings drop and long-term effects, the military may need to supplement military training with formal education, particularly in fields that require credentials in the civilian labor market.

First we describe the sample used for this analysis and the methodology for estimating the earnings differences controlling for baseline characteristics. We then present results by YOS, followed by results by MOS.

Sample and Data for Earnings Analysis

The primary sample used in this analysis consists of individuals who applied for enlistment to the military and who met the typical enlistment requirements.[6] In particular, we focus on individuals who were 17 years old and older at the time they applied for military service, had no prior military service, scored at or above the 31st percentile on the AFQT, had no postcollege schooling, and had no potentially disqualifying health conditions or potentially disqualifying drug or alcohol use. Although our data contain individuals who applied throughout the 1990s and early 2000s, our primary analyses focus on individuals who applied in 1994. This is because this is the first cohort for which we have information on military earnings and can thus form a measure of total earnings that includes payments that are not subject to Medicare taxation. Thus this cohort gives the longest follow-up window for which total earnings are observed in every year following application.

[6] See LMMK for a detailed description of the military application process, including the standards the military typically requires recruits to meet in order to be eligible to serve in the military.

The main outcome we used is annual total cash compensation plus a measure of tax advantage that applies to non-taxable military income. This measure was computed by summing annual earnings reported to SSA, military pay not subject to Medicare taxation, and the tax advantage. The details of how total annual earnings were computed are discussed in LMMK. In addition to examining results for total earnings measured in 2005 dollars (deflated using the Consumer Price Index), we also examined results expressed as a percentage of mean earnings for individuals in the sample in a particular year after application.

As in LMMK, we defined an applicant as having enlisted if an individual accessed (according to MEPCOM records); i.e., the military inducts the individual into military service. However, unlike LMMK, we further stratified the enlistees by YOS and MOS. Information on MOS and YOS comes from the Proxy PERSTEMPO file. For each service member, we identified the year an individual separated from the military as the first year an individual had at least one month with missing pay-grade information on the Proxy PERSTEMPO file and with missing pay-grade data in all of the following year. YOS was then defined as the difference between the separation year and the year of enlistment. Note, however, that a sizable fraction of active-component enlistees will continue to serve in the reserve components, so separation from active-component service does not necessarily mean separation from military service altogether. MOS for this analysis is based on the first primary MOS a service member enters.[7]

Tables A.1–A.4 in the appendix of this report show summary statistics for the individuals in the 1994 application cohort that form our main analysis sample. Sample means are depicted overall, for nonenlistees, and by YOS (Tables A.1–A.2) and MOS (Tables A.3–A.4). Several factors are worth noting. First, many enlistees separate after less than one YOS (in fact, it is the most common length of service). Second, after eight YOS, the separation rate falls sharply. Because of the small sample size for the higher YOS values, there are many cases in which we needed to drop cells from the analysis or in which there were too few enlistees or nonenlistees. This problem can be seen in the percentage of applicants in the full data who are represented in the earnings analysis. When stratifying the data by MOS, the most notable pattern is that "combat arms" is the largest occupational category by a significant margin.

Methodology for Earnings Analysis

The methodology used to analyze the relationship between enlistment and earnings follows that pioneered by Angrist (1998) and used in LMMK that compares the outcomes of applicants who enlist with the outcomes for those who do not enlist, controlling for the information on

[7] The primary MOS is the occupation in which an individual receives training. At times, a service member needs to serve in an occupation other than the one in which he or she was trained; the MOS in which he or she actually serves at a given point in time is known as the duty MOS. We focus on the primary MOS because that reflects the occupation for which a service member receives the most training and hence might be most relevant for finding postmilitary employment.

military application records. As argued by Angrist (1998) and in LMMK, these controlled contrasts might identify the causal effect of military service because of the focus on military applicants and the quality of the covariates available on application records. This is because enlistees are likely to be more similar to applicants who do not enlist than to individuals in the general population (both in terms of observable factors, such as age and AFQT, and in terms of unobservable factors, such as willingness to follow orders).[8] Further, the application records contain all the information the military observes about applicants, so controlling for these factors accounts for any confounding due to the military screening out unqualified applicants.

To implement this approach, Angrist (1998) and LMMK used exact covariate matching. In particular, in a cell *c*, defined by a vector of covariates *X*, differences in mean earnings between enlistees and nonenlistees were computed. These cell-level estimates of the treatment effect were then aggregated using the share of enlistees in cell *c* as the weight to form an estimate of the average effect of enlistment among the enlistees.[9]

The current project attempted to go a step further by comparing veterans who remain in the military for a given length of time or who serve in a particular occupation with nonveterans. To do this, the sample of enlistees is restricted to a particular YOS value (or occupational category, for the analysis by MOS). The covariates used to form cells include age, race (white, Hispanic, black, and "other"), gender, AFQT category (3b, 3a, 2, 1), service, and a dichotomous measure of preapplication earnings (i.e., an indicator variable for having earnings above the median in a given cell).

The viability of this approach for estimating the overall effect of enlistment (i.e., not for a particular YOS or MOS) depends on the ability of the covariates to control for other factors that are related to enlistment and earnings. For this approach to provide valid estimates of the overall effect of enlistment for a particular MOS or YOS, the baseline covariates must also control for factors related to occupational sorting and duration in the military.

As described above, a strong case can be made for thinking that this basic approach might yield estimates of the overall effect of enlistment with minimal bias. Even still, it is important to recognize that there still may be selection bias if the applicants who decide not to enlist make that decision for reasons that are related to earnings later in life (e.g., because tastes for military service are correlated with earnings opportunities). These concerns about selection bias are even stronger when estimating the effects of enlistment by MOS and especially YOS. This is because both occupation and YOS are affected by factors that arise after the military conducts its initial screening.

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[8] See LMMK for a description of the military recruitment process and military enlistment standards.

[9] The variance of the weighted average of the treatment effect was computed as the sum of the product of the cell-level variance and the square of the weight applied to this cell when computing the aggregate estimate of the treatment effect.

For example, consider the case of someone who remains in the military for only one year. Because first-term military contracts are all at least two years and almost always at least three years, service of only one year could be indicative of someone leaving the military because he or she was performing poorly. If poor military performance is an indication of other problems in the labor market, and if the baseline covariates do not fully control for these problems, then estimates for enlistees with one YOS might be downward biased for the effect of enlistment for these individuals. Conversely, if completing one's first-term contract is an indicator of positive labor market attributes that are not fully accounted for by baseline covariates, then estimates of enlistment for these individuals could be upward biased.[10] Similar reasoning suggests that estimates for a particular occupational category might be biased (although we cannot say whether any such bias would be larger or smaller than the bias when looking at results by YOS).

Because of these limitations of the analytic approach, we view our estimates as suggestive rather than causal. Nonetheless, the question of how length of service and one's occupation while serving relate to the economic returns to military service is a critical one for understanding how military service affects economic well-being and is one about which we know very little. Thus, this analysis is still of considerable interest in spite of it being more suggestive than causal.

Results for Earnings Analysis

We begin the discussion of the results for earnings by presenting findings based on the full sample of applicants (i.e., not disaggregating by YOS or MOS). We use these findings to summarize the main conclusions in LMMK. Next, we show results by YOS and then by MOS. As discussed above, YOS and MOS might be endogenous to labor market outcomes in a way that the baseline characteristics available on the application records might not fully account for. Nonetheless, the findings are of considerable interest in helping to understand what drives the pooled results.

Results for Pooled Applicant Sample

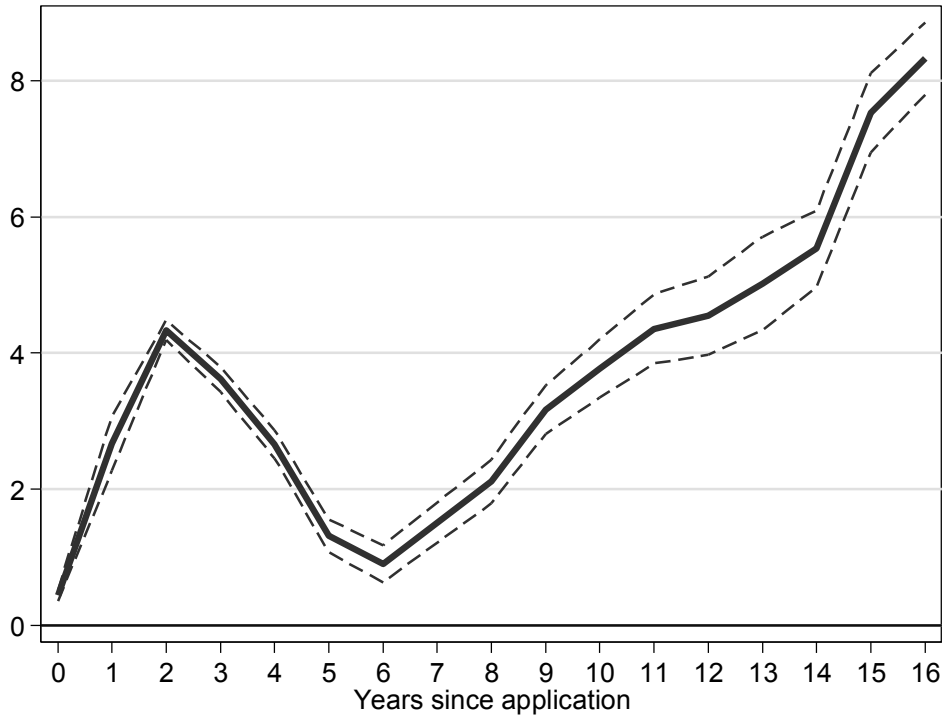
Figure 3.1 shows the estimated effect of enlisting in the military on annual earnings among individuals who applied to the military in 1994. The horizontal axis represents the years since application (in this case, years since 1994, ending in 2010). The vertical axis represents the effect of enlisting in the military. The dashed lines are the 95-percent confidence intervals around the point estimates.

[10] To illustrate the differences in baseline characteristics of veterans who have only a few YOS compared with those who remain in the military for a fairly long time, we examined average AFQT scores for the 1994 application cohort. The average AFQT percentile score was about 2 percentage points lower among those with less than three YOS at the time of separation than it was among those with three to six YOS. Those with more than six YOS, on the other hand, had average AFQT percentile scores that were about 2 percentage points higher than those with three to six YOS had.

The patterns in Figure 3.1 mirror those reported in LMMK. In the first three years after applying to the military, enlistment is associated with a strong positive earnings gain of about $3,000 to $4,000. This short-term effect peaks in year 2 and declines through year 6. Thereafter, it steadily increases. By year 16, military service is associated with an average effect of about $8,000.

LMMK put forth an explanation for these patterns related to the timing of separation from the military and the effect of separation on earnings. In particular, the strong short-run effect, over the period when most veterans are still in the military, is consistent with the military paying a compensating wage differential to service members while they are serving in the military. It also reflects veterans having a full-time job while in the military while many of their civilian counterparts are in college or not in full-time employment. Then over the next set of years (years 4-8) when many veterans separate from the military, the veterans' earnings premium falls (although it remains positive). This could be due to veterans entering the civilian labor market with less civilian work experience than nonveterans, and it could also reflect delayed entry into college (the effects of military service on educational attainment are covered in Chapter Four). In the longer run, veterans earn significantly more than comparable nonveterans. This might be due to veterans "catching up" to nonveterans in terms of civilian work experience and college completion. However, some evidence in LMMK suggests that it is due at least in part to a very large earnings differential for the small number of veterans who remain in the military. In the analyses that follow, we explore this hypothesis in greater detail by breaking out the returns shown in Figure 3.1 by YOS (including results for individuals who never separate from the military). We further unpack these results by investigating how the returns presented below vary by the occupation that veterans enter.

Figure 3.1
Estimated Effect of Enlistment on Annual Earnings (2005 dollars), by Years Since Application, 1994 Application Cohort



NOTE: Dashed lines represent 95-percent confidence intervals around point estimates.

Results by Years of Service

Figure 3.2 shows effects of military enlistment for select YOS values, expressed in 2005 dollars. Figure 3.3 shows these same effects expressed as a percentage of mean earnings. The results for the complete set of YOS values are reported in Tables A.5–A.8 in the appendix. One common feature across all YOS values (for the individuals who actually separate from the military) is that earnings fall sharply in the year of separation. This lends support to the conjecture in LMMK that the military pays a compensating wage differential to veterans while they are in the service. However, it could also reflect the end of full-time employment, competing against nonveterans who have more civilian work experience, and veterans commencing college in the year in which they exit the military. Although the results in Chapter Four suggest that the college-entry explanation might explain part of the drop in the military earnings premium at the time of separation, we cannot determine the relative importance of the compensating wage differential and the other explanations for this phenomenon.

Figure 3.2
Estimated Effect of Enlistment on Annual Earnings, by Years Since Application and Years of Service Prior to Separation, 1994 Application Cohort

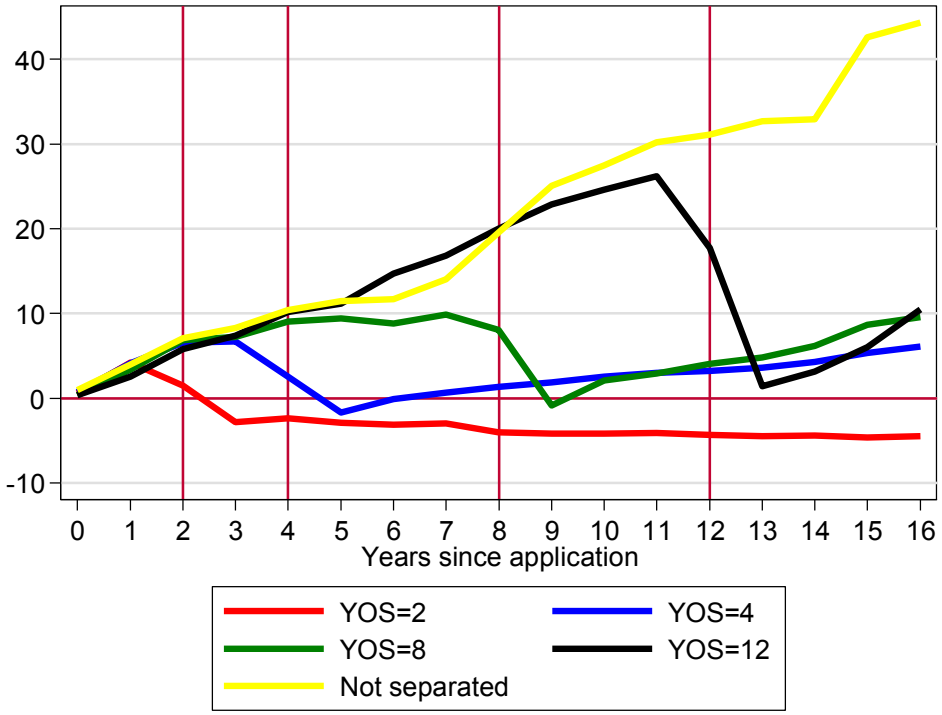
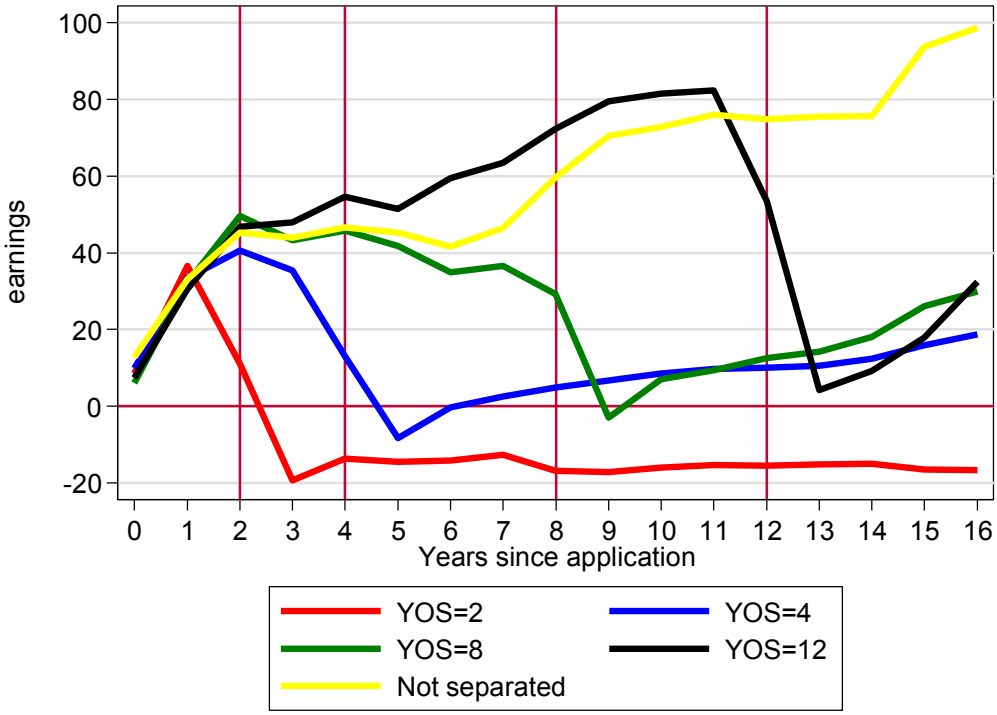


Figure 3.3
Estimated Effect of Enlistment on Annual Earnings (as a Percentage of Mean Earnings), by Years Since Application and Years of Service Prior to Separation, 1994 Application Cohort



Another very interesting pattern is that the earnings premium that arises while individuals are in the military is quite consistent across different YOS values through about year 3 or 4. This likely reflects the relative lack of variation in earnings while in the military, at least early in one's military career. After this point, though, the earnings premium while in the military diverges across YOS, with smaller premiums for individuals who separate earlier. This is not surprising. It could reflect that a faster rate of promotions is predictive of staying in the military (Hosek and Martorell, 2009), or it could reflect higher earnings among the nonveterans with whom the veterans are being compared.

Despite these common features, the results clearly indicate that the average effects reported in Figure 3.2 mask considerable heterogeneity. For instance, individuals who separate after one YOS (see Tables A.7–A.8 in the appendix) never have an earnings premium relative to nonveterans, even in year 0 and year 1, when these veterans would have been in the military.[11] In all subsequent years, they have sizable earnings losses relative to their nonveteran peers. To be

[11] Note that most individuals who separate after one YOS will not be in the military for all of year 0 or all of year 1. This implies that any earnings premium associated with actually being in the military will be muted for this group in years 0 and 1.

clear, this is not necessarily evidence that military service exerts a negative effect on the earnings of individuals who serve for less than two years. Because the typical first-term contract length is usually at least three years, YOS less than three is a strong indicator of separation before the end of one's first contract.[12] If failure to complete the terms of the first contract is correlated with other factors that could impair earnings in the civilian labor market, then the negative long-run earnings effects could reflect selection bias.

For other YOS values, the long-run estimates are positive and tend to increase with time. For YOS 4–12, by year 16, the estimated returns are about 20–30 percent of mean earnings. The returns are especially large for individuals who do not separate, with year 16 earnings nearly double what they are for nonveterans with similar applicant characteristics.[13] Although these long-run earnings estimates are quite large, it is also important to keep in mind that they might also reflect selection bias. In particular, individuals who remain in the military for four or more years might be *positively selected*, i.e., have other factors that civilian employers value.[14] Thus, we are cautious about concluding that all of the long-run returns should be attributed to military service per se.

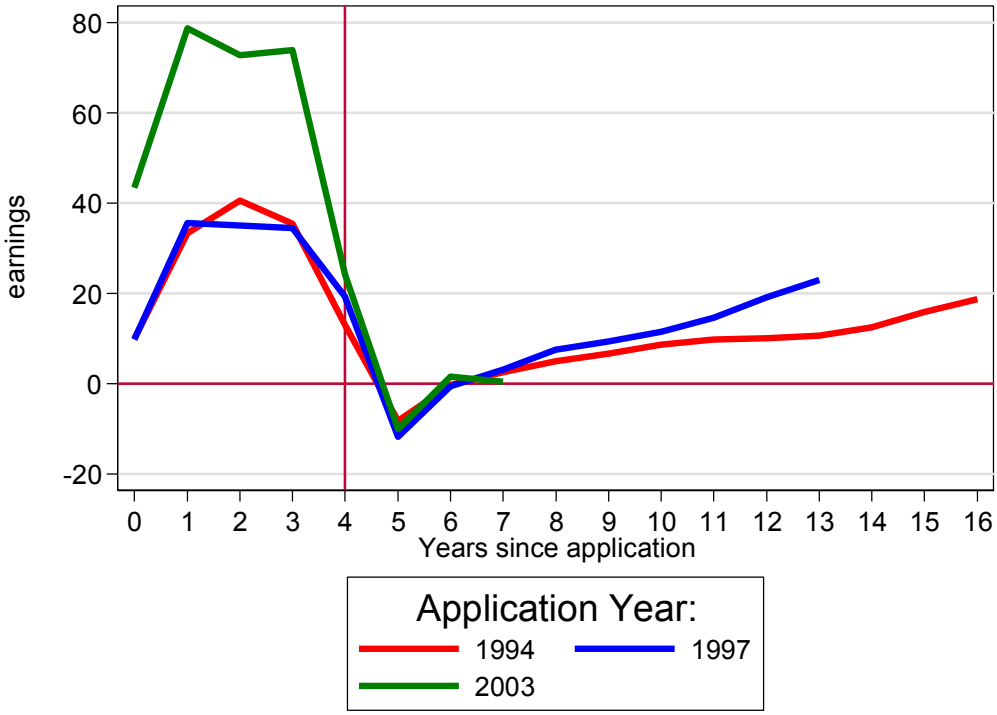
We also examined whether the results vary by application cohort. Figure 3.4 shows returns for those who separate from the military after four YOS for the 1994, 1997, and 2003 application cohorts. For the more-recent cohorts, we have fewer follow-up years. Nonetheless, it is clear that, across all the cohorts, there is a large return to being in the military in the years of active-duty military service. This premium falls sharply upon separation; however, several years after separation, the estimated returns become positive again and trend upward with time.

[12] For instance, for the individuals who first entered the military in 1994, about 80 percent have a first contract at least three years long.

[13] In this analysis, we observe separations through 2007, so some individuals in the "did not separate" group will have separated in years 14–16. However, because separation rates conditional on remaining in service 14 years are so low, virtually no individuals in this group will separate in or before 2010.

[14] For instance, individuals who separate after one year have lower AFQT scores than do individuals who separate after more than one YOS. We control for these (and other) differences, but we suspect that we are not controlling for all relevant factors.

Figure 3.4
Estimated Effect of Enlistment on Annual Earnings, by Years Since Application and Application Cohort, Separated After Four Years of Service



Results by Military Occupational Specialty

Figures 3.5 and 3.6 show estimated effects of military enlistment by a service member's first MOS for four different occupational groups. Results for the complete set of one-digit MOS groups are shown in Tables A.9–A.12 in the appendix. Note that these results pool across YOS.[15] Thus at any point in time, the estimates should be thought of as the difference between the earnings of veterans who go into a particular MOS and the earnings of comparable nonveterans, where some of the veterans are still serving in active duty and others have exited. Of course, the fraction of veterans still in active duty declines with time since application.

[15] Recall that our estimates are based on exact cell matching. If we were to stratify the enlistees by YOS and MOS, we would be forced to drop too many small cells.

Figure 3.5
Estimated Effect of Enlistment on Annual Earnings, by Years Since Application and Military Occupational Specialty, 1994 Application Cohort

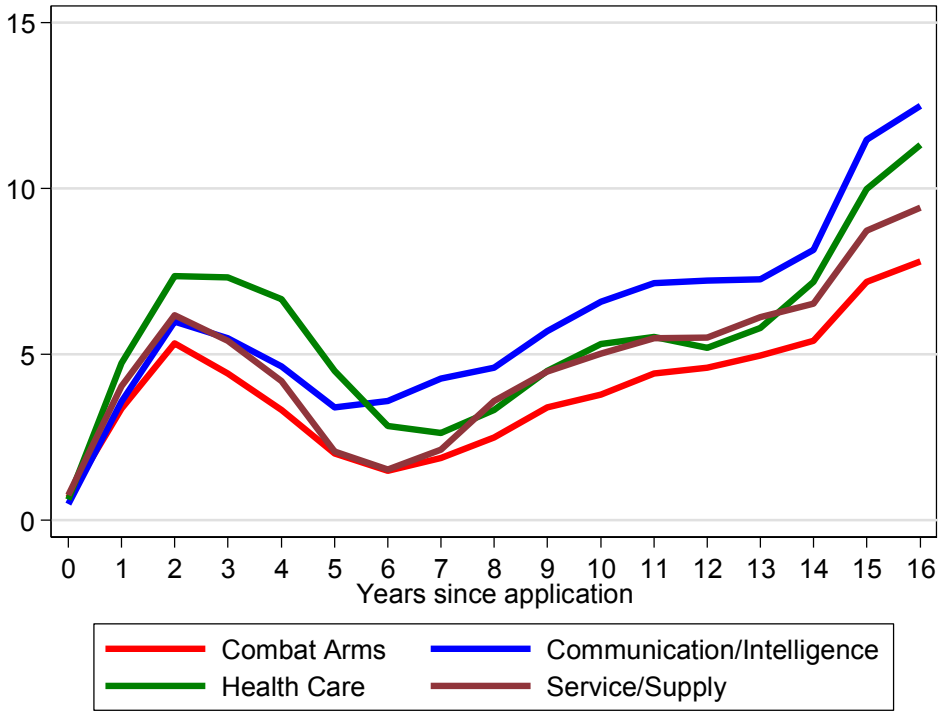
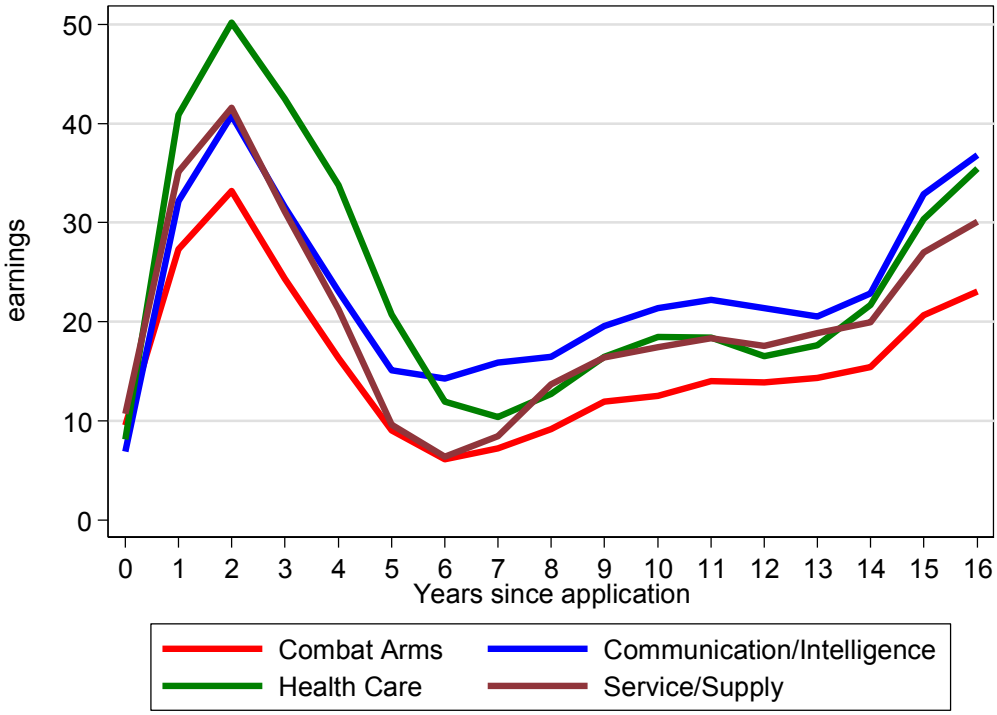


Figure 3.6
Estimated Percentage Effect of Enlistment on Annual Earnings, by Years Since Application and Military Occupational Specialty, 1994 Application Cohort



The results reveal several interesting patterns. First, the same basic time trend seen in Figure 3.1 can be seen for all occupations. For all occupations, military service is associated with a sizable earnings premium in the first several years after applying to the military; it declines over the next several years (but remains positive) and then increases with time, starting around year 7. Crucially, for all occupational categories, military service is associated with sizable long-run earnings gains.

Despite these commonalities, there are some notable differences across occupations. In the short run, the largest premiums were found for veterans who worked in health care occupations (around 50 percent of mean earnings in year 2) and were smallest for those in combat arms (around 30 percent of mean earnings in year 3). This heterogeneity is somewhat surprising given that, in the short run, there is little earnings dispersion among active-component service members.[16] One possible explanation for the divergence in the short run is that the earnings of nonveterans who are comparable to veterans who go into health care occupations are lower than those of nonveterans comparable to veterans who go into combat arms. This might be the case

[16] For instance, across one-digit occupational categories, the percentage with four YOS ranges from 20 to 25 percent for all but one occupational category (communications or intelligence, in which 27 percent separate after four years).

because the nonveterans most like veterans in health care occupations are more likely to go to college (and hence have lower earnings in the short run) than are nonveterans comparable to veterans who go into combat arms. Another possibility has to do with differences in separation rates across occupations (discussed below).

In the long run, we also find some differences in the earnings gains associated with military enlistment. Veterans who enter occupations in health care or communications or intelligence have larger gains relative to comparable nonveterans than do veterans who go into combat arms (about 35 percent, compared to 23 percent, respectively), and the earnings gains for veterans who go into service or supply occupations fall in between these two other groups. The estimates for the complete set of occupations are listed in Tables A.9–A.12. Individuals in craftswork have relatively low earnings gains (similar to combat arms). Like service and supply, the returns for "other/allied" occupations, along with mechanical repair, fall in the middle of what we find across the occupational categories. Those in functional and administration occupations have the highest returns, along with those in health and communications and intelligence.

Again, it is hard to say exactly what is causing these differences, but we do have some possible explanations. One is that the skills acquired in some military occupations transfer more easily to the civilian job market than others. This is plausible given the patterns we see. In particular, it is reasonable to suspect that veterans who go into combat arms will acquire fewer skills civilian employers value than would veterans who go into occupations in health care or communications. Another possibility is related to the fact that veterans who separate later, or not at all, experience much larger earnings gains associated with enlistment than do veterans who separate after fewer YOS (as discussed above). For instance, 61 percent of individuals in combat arms separate within four years, compared with 38 percent in health care. However, the rate of separation within eight years does not exhibit as much variation, with about 80 percent separating within eight years in most occupational categories, with the lowest (72 percent) for health care.

In summary, then, our findings for the pool of all applicants mirror the findings reported in LMMK. A strong earnings gain of about 3 to 4 percent of mean earnings peaks in year 2 after enlistment and declines through year 6. Thereafter, it steadily increases, and, by year 16, military service is associated with an average effect of about 8 percent of mean earnings. For all YOS values, there is a large return to being in the military in the years of active-duty military service. This premium falls sharply upon separation; however, several years after separation, the estimated returns become positive again and trend upward with time. Finally, for all occupational categories, military service is associated with sizable long-run earnings gains. Veterans who enter occupations in health care or communications or intelligence have larger gains relative to comparable nonveterans than do veterans who go into combat arms. The earnings gains for veterans who go into service or supply occupations fall in between these two other groups.

The degree to which skills are transferable to the civilian labor market almost surely varies by occupation. For instance, veterans who learn lots of skills in, say, truck repair or

communication technology, might have skills that are very valuable to civilian employers. However, even though some occupations have transferable skills, civilian employers might not understand the military occupation labels. We suggest that a military credentialing program would help with this problem. A formal credential or certificate that describes service members' course of study and range of skills could help civilian employers understand the value that veterans can bring to the workplace regardless of their MOS.

Chapter Four. Analysis of the Effect of Military Service on Education

This chapter presents results on the differences in educational attainment between veterans and comparable nonveterans stratified by YOS prior to separation and by first MOS. We begin by describing the sample used for this analysis and the methodology for estimating the differences in educational attainment, controlling for baseline characteristics. We then present results by YOS, followed by results by MOS.

Sample and Data for Education Analysis

Our education data come from the National Student Clearinghouse, a nonprofit organization that contracts with institutions of higher education to verify college enrollment and degree receipt for student-loan agencies. The Clearinghouse data allow us to track military applicants as they enter and complete college at institutions nationwide.

The National Student Clearinghouse maintains college-enrollment data for institutions in years in which those institutions had an active contract with the Clearinghouse. Between 1993 and 2010, the Clearinghouse's coverage of college enrollment grew from 13 to 93 percent of total college enrollment nationwide (see LMMK for details). The Clearinghouse also maintains an optional degree-verification service that is used by the majority of contracting institutions. In 2010, about 70 percent of all U.S. college students attended an institution that participated in this service. Participating institutions submit retrospective records on degree receipt for as far back as those records are kept electronically. Consequently, for earlier years, coverage of college degrees is more complete than coverage of college enrollment. The Clearinghouse is able to verify about 63 percent of all degrees awarded by nationwide in 1991, the earliest applicant cohort employed in these analyses.

As described in LMMK, incomplete Clearinghouse coverage of enrollments and degrees informed our analysis and sample selection. First, because of increasing coverage rates over time, there is a trade-off between data coverage and the ability to observe completed college enrollment and degree attainment. Selecting earlier applicant cohorts increases the likelihood that we will observe completed college enrollment and degree attainment. However, the Clearinghouse data omit a larger fraction of enrollments and degrees attained in the earlier years of this sample. Selecting later applicant cohorts provides better coverage but allows us fewer years to observe completed enrollment and degree attainment. This trade-off is more pronounced for college-enrollment outcomes, the coverage of which increased sharply between 1993 and 2010.

Second, because enlistees delay college enrollment relative to nonenlistees, we must restrict the Clearinghouse data to colleges that are in every year of our sample. This ensures that, a priori, enlistees and nonenlistees have equal opportunity to appear as enrolled in these data.

Given these factors and the direct cost of obtaining data from the National Student Clearinghouse, we restricted our sample as follows. When examining college enrollment, we selected a sample of 120,000 military applications from the 1998–2000 applicant cohorts. *Enrollment* is defined as enrolling in a college that began contracting with the Clearinghouse prior to 1998. When examining college degree attainment, we select a sample of 120,000 military applicants from the 1991–1994 applicant cohorts. *College degree attainment* is defined as attaining a degree from a college that was contracted with the Clearinghouse at the time we obtained Clearinghouse data (March 2010) and that submitted degree-attainment data covering the period 1991–2010. We maximized our statistical power by choosing our samples to get equal numbers of enlistees and nonenlistees in each cohort. In order to ensure a large enough sample to detect reasonable effect sizes for well-defined subgroups, we also stratified our sample by race and AFQT category, oversampling high-aptitude Hispanics and African Americans while undersampling low-aptitude white applicants.[17]

Methodology for Analysis of Educational Attainment

The methodology we used to analyze the relationship between enlistment and educational attainment is identical to the methodology we used in LMMK and similar to the methodology we used to analyze the relationship between enlistment and earnings. The identification strategy follows that pioneered by Angrist (1998) and used in LMMK, comparing the college enrollment and degree attainment outcomes of military applicants who enlist with those of applicants who do not, conditional on their characteristics.

There are two key ways in which the analysis of educational attainment differs from that of earnings. First, because of limited sample sizes of service members for whom we can observe educational attainment, we were unable to use the explicit matching approach that we used for earnings models. Instead, we needed to use standard regression methods to correct for differences in baseline characteristics of enlistees and nonenlistees. Second, the outcomes in our educational attainment models were binary indicators of whether a service member enrolled in or

[17] See LMMK for details on the stratification methodology. Employing these sample restrictions, we measured 67 percent of all college enrollments for the 1998–2000 applicant cohorts and 63 percent of all awarded college degrees for the 1991–1994 applicant cohorts. These statistics imply that we underestimated college enrollment and degree attainment by approximately one-third. This underestimation poses a problem for our estimates of the effect of enlistment on education only insofar as applicants who do or do not enlist are more or less likely to attend and receive degrees from the colleges that are not in our sample. One possible concern is the fact that the National Student Clearinghouse data cover only 20 percent of enrollments at for-profit colleges for our cohorts. The evidence suggests that veterans have a high propensity to enroll at for-profit institutions, which would cause us to underestimate the impact of enlistment on college enrollment and completion overall.

completed college, so we used standard probit models to isolate the impact of military service on educational attainment.

Specifically, we estimated the effect of enlistment on educational outcomes employing the following probit model:

$$\Pr\left(Y_{it}=1\right)=\Pr\left(a_t+B_tD_i+X_iG_t+e_{it}>0\right), \quad 4.1$$

where Y_{it} is an indicator for whether applicant i was enrolled (or had ever enrolled) in college or obtained a college degree in the tth year following application, D_i is an indicator for whether the applicant enlisted, X_i is a vector of applicant characteristics,[18] and e_{it} is an idiosyncratic, normally distributed error term. Y_{it} is defined for any two- or four-year college enrollment or degree.

As with earnings, the goal of the current project was to push the models presented in LMMK a step further by comparing veterans who remain in the military for a given length of time or who serve in a particular occupation with nonveterans. To do this, we included interaction effects between D and YOS (or MOS) in Equation 4.1 above. In particular, to compare educational attainment by YOS, we modified Equation 4.1 as follows:

$$\Pr\left(Y_{it}=1\right)=\Pr\left(a_t+B_t\times Y03_{it}\times D_i+B_t\times Y47_{it}\times D_i+B_t\times Y8P_{it}\times D_i+X_iG_t+e_{it}>0\right), \quad 4.2$$

where $Y03_{it}$ is an indicator for whether an applicant separated during the first three years of service, $Y47_{it}$ is an indicator for whether applicant i separated between the fourth and seventh YOS, and $Y8P_{it}$ is an indicator for whether applicant i was still active during the eighth YOS. We used a similar approach to compare educational attainment by MOS, but we included interaction terms for the service member's initial MOS instead of YOS at separation.

Our approach for estimating the relationship between enlistment and educational attainment by MOS and YOS suffers from the same methodological concerns as the earnings models presented in Chapter Three. Specifically, we expected that, conditional on enlisting, service members who choose different MOS or separate with different YOS may differ in ways that we cannot capture with the characteristics included in X_i. See Chapter Three for details.

Results for Pooled Applicant Sample

Figure 4.1 shows the estimated effect of enlisting in the military on the cumulative probability of having ever enrolled in college by year since first visiting the MEPCOM station among individuals who applied to the military between 1998 and 2000.[19] The horizontal axis represents the years since application. The vertical axis represents the effect of enlisting in the military, expressed as a percentage of mean earnings. The dashed lines are the 95-percent confidence intervals around the point estimates.

[18] We use the same applicant characteristics as we did in the earnings models described in Chapter Three.

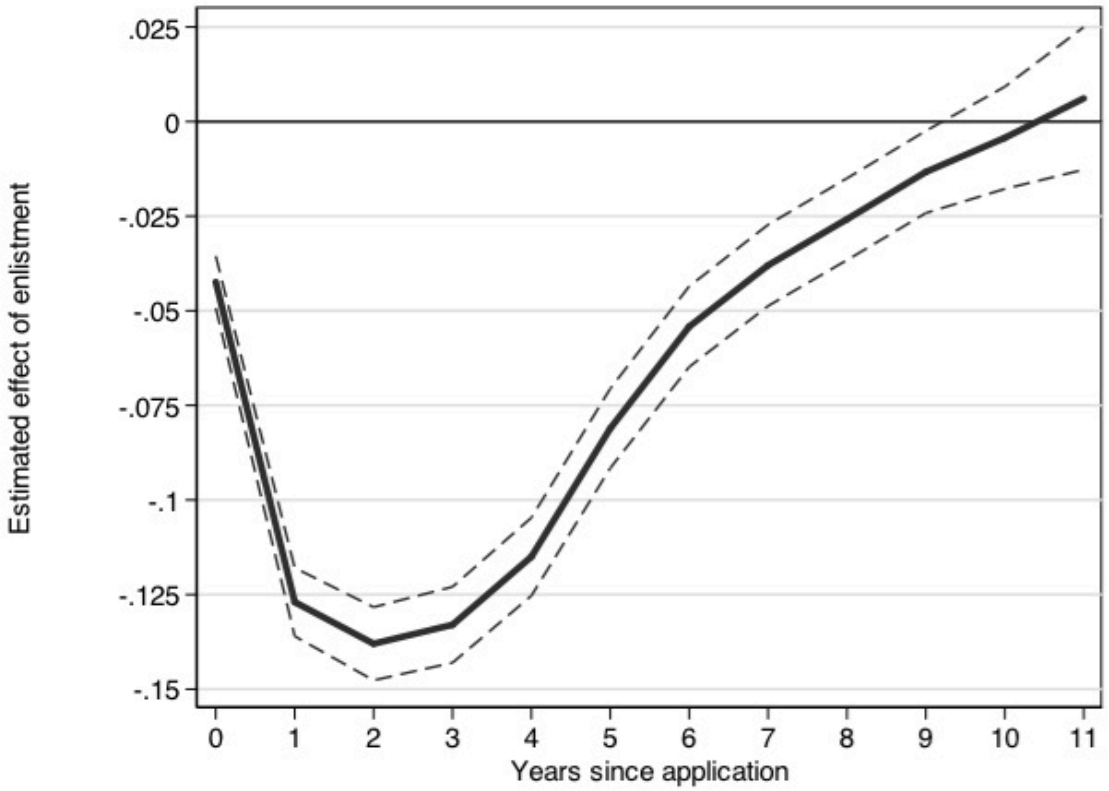
[19] The point estimates and standard errors are included in LMMK.

The patterns in Figure 4.1 demonstrate that joining the military causes service members to significantly delay their college enrollment, but they are equally likely to enroll in college at some point during their lives as similar nonenlistees. Enlistees are 13 percentage points less likely to have ever enrolled in college by the second postapplication year, but, by the ninth postapplication year, there is no discernible difference in the probability of having ever enrolled in college between enlistees and similar nonenlistees. Moreover, enlistees start to enroll at higher rates than nonenlistees during the 11th postapplication year, the last year in which we could observe college enrollment for any of the cohorts we studied, which suggests that, if we could observe college enrollment outcomes later in life, we might find that service members eventually enroll in rates that are statistically higher than those of nonenlistees.

Figure 4.2 shows the estimated effect of enlisting in the military on degree completion by year since first visiting the MEPCOM station among individuals who applied to the military between 1991 and 1994.[20] The results are similar to those for college enrollment, suggesting that joining the military significantly delays college completion but, over time, service members catch up to their civilian counterparts in terms of college-completion rates. In particular, we see that service members are 4 percentage points less likely than similar nonenlistees to have completed a college degree by the fifth postapplication year but that the college-completion rates of similar enlistees and nonenlistees are statistically indiscernible by the 12th postapplication year. As with college enrollment, service members start to complete college degrees at higher rates than nonenlistees during the 15th and 16th postapplication years, the last years for which we observed degree completion, suggesting that we may detect positive impacts of military enlistment on college degree completion with a longer time horizon. The longer time horizon for observing positive impacts on degree completion relative to college enrollment is consistent with the fact that service members enrolling in and completing college after separating from military service.

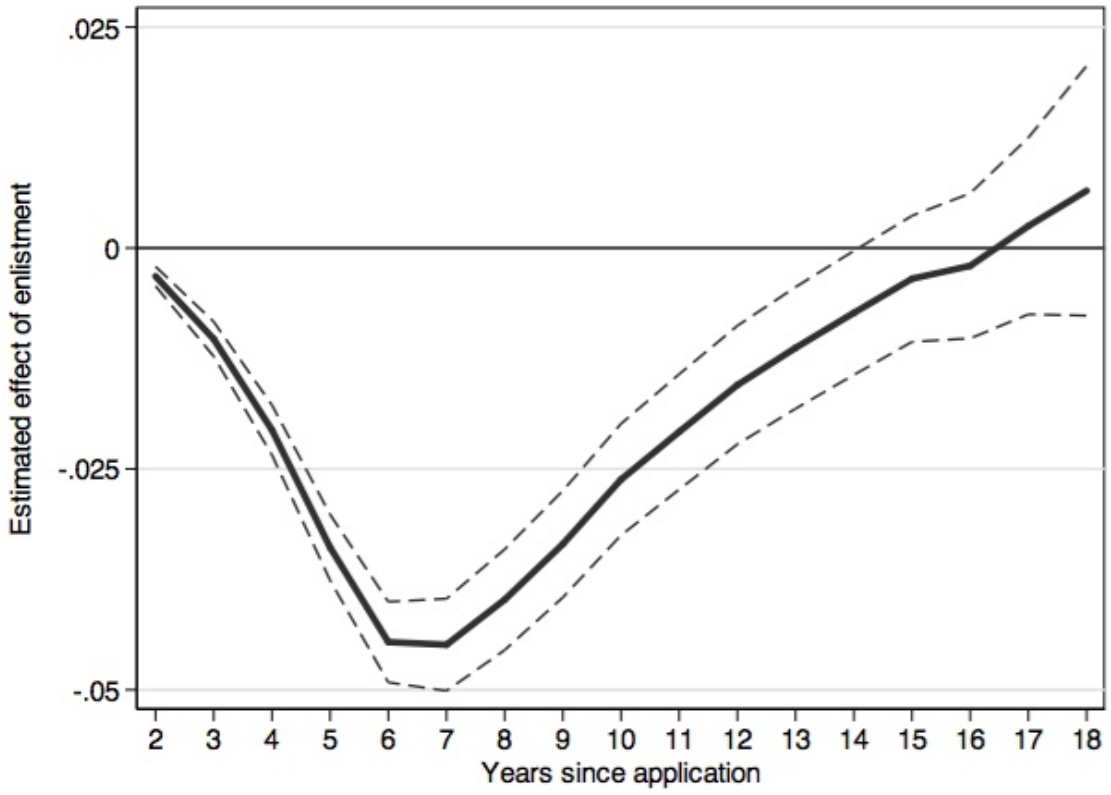
[20] The point estimates and standard errors for these models are reported in LMMK.

Figure 4.1
Estimated Effect of Enlistment on Cumulative College Enrollment, by Years Since Application, 1998–2000 Application Cohorts



NOTE: Dashed lines denote 95-percent confidence intervals.

Figure 4.2
Estimated Effect of Enlistment on College Degree Attainment, by Years Since Application, 1991–1994 Application Cohorts



NOTE: Dashed lines denote 95-percent confidence intervals.

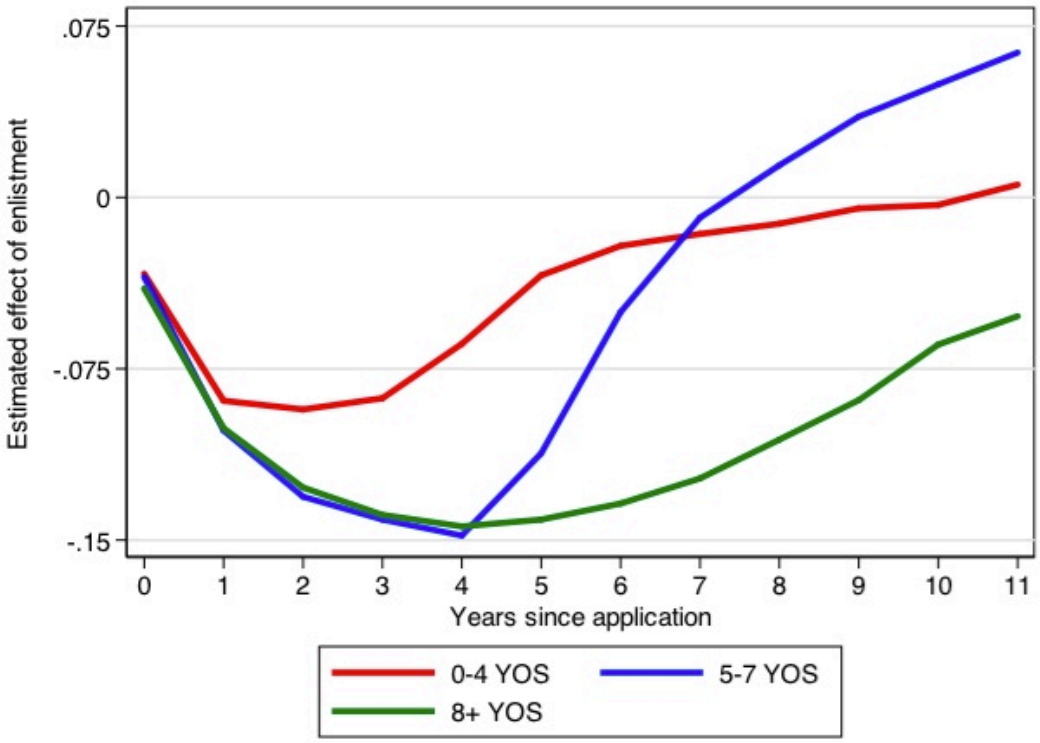
College Enrollment Patterns by Years of Service

Figure 4.3 shows the effects of military enlistment on college enrollment by YOS. The point estimates and standard errors for these models are included in Table A.13 in the appendix. There are two interesting patterns in the results. First, the results suggest that service members who accumulate five to seven YOS before separating are significantly more likely than similar nonveteran applicants to enroll in college at some point during their lives. Although these service members delay college enrollment and are 15 percentage points less likely to have enrolled in college by the fourth postapplication year, they catch up with and begin to surpass their nonveteran peers by the seventh postapplication year. By the 11th postapplication year, service members who separate with five to seven YOS are 7 percentage points more likely than similar nonveteran military applicants to have ever enrolled in college. In contrast, service members who accumulate eight or more YOS are significantly less likely than similar nonveterans to enroll in college by the 11th postapplication year, and service members who accumulate less than four YOS do not catch up with similar nonveterans until the 11th postapplication year.

Because of concerns about selection into timing of separation from military service (discussed in Chapter Three), we urge caution in interpreting the results presented in Figure 4.3. Nevertheless, these patterns suggest that service members who serve five to seven years and then separate from military service may be the most likely to use their education benefits and enroll in college. Relative to those who separate with eight or more YOS, these service members are younger and less likely to be married and have children at the time of separation. They also have less military experience that can substitute for education in the labor market. For these service members, military service may effectively act as a gateway to college. In contrast, service members who separate with eight or more YOS may have better job prospects at the time of separation, or college may be less desirable because of the need to cover family expenses. Although it is tempting to say that these service members use military as an alternative to college, our results suggest that many of them do still enroll in college. Finally, the results suggest that service members who separate with four or less YOS delay their college enrollment significantly longer than those who separate with five to seven YOS but that they catch up with their civilian counterparts and do so more quickly. These service members may be of lower quality than applicants who separate with more YOS because most initial contracts are for three or more years, so most of these service members separated prior to the end of their initial contracts.

Second, the results also demonstrate that service members who enroll in college tend to do so soon after separating from military service. This is particularly true for service members who separate with five to seven YOS. Although these service members are 15 percentage points less likely than similar nonveterans to have enrolled in college by the fourth postapplication year, they are equally likely to have done so by the seventh postapplication year, suggesting that these service members enroll in college in high proportions immediately after separation. This suggests that college entry might explain part of the temporary drop in the military earnings premium at the time of separation, which we observed in Chapter Three.

Figure 4.3
Estimated Effect of Enlistment on Cumulative College Enrollment, by Years Since Application and Years of Service Prior to Separation, 1998–2000 Application Cohorts



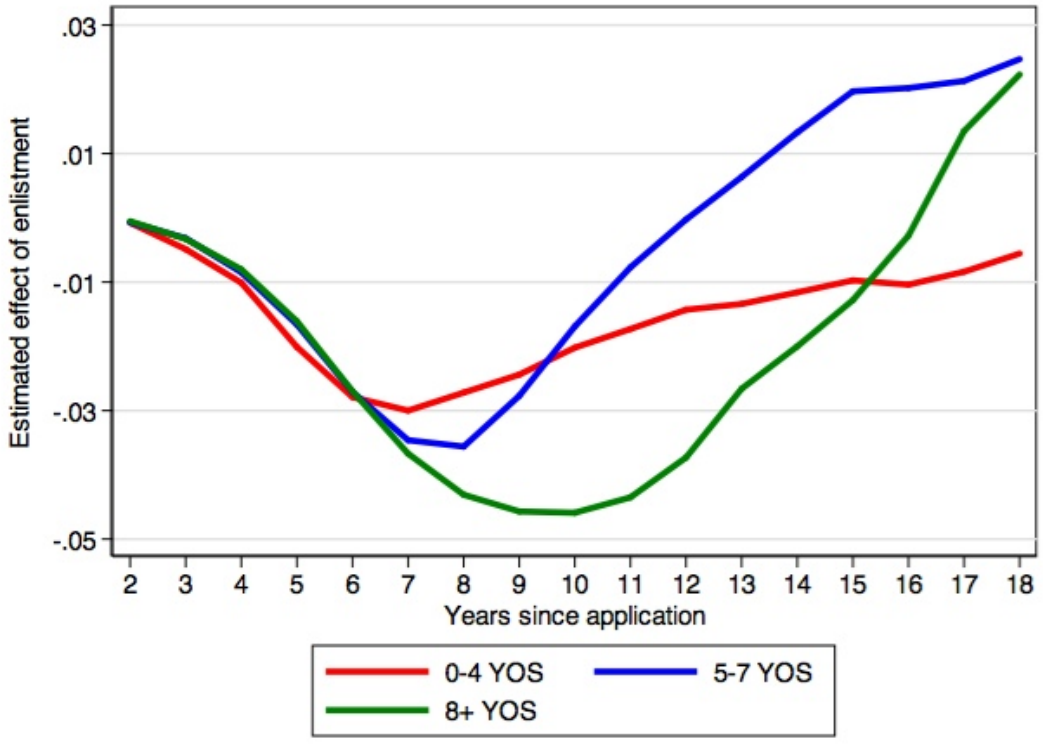
College Degree–Attainment Patterns by Years of Service

Figure 4.4 shows the effects of military enlistment on college degree attainment by YOS. The point estimates and standard errors for these models are included in Table A.13 in the appendix. With two key exceptions, the results largely mirror those for college enrollment. First and most obvious, the patterns for degree attainment generally lag those for college enrollment, reflecting the fact that college takes time for students to complete. For example, although service members who separate with five to seven YOS begin to catch up with similar nonveterans in terms of college-enrollment rates by the fourth postapplication year, this does not occur until the sixth postapplication year for degree attainment.

Second, although the results in Figure 4.3 suggest that service members with eight or more YOS are less likely than similar nonveterans to enroll in college over the course of their lives, the results here suggest that they eventually surpass similar nonveterans in terms of degree attainment. There are several possible explanations. First, the time horizon for which we observe college enrollment is shorter than that for which we observe degree attainment, 11 versus 16 postapplication years—a difference of five years. Although there is a lag in degree attainment relative to college enrollment, most college degrees are earned by the fifth year, suggesting that

we should observe more service members who have completed their education in the degree models. Moreover, we showed in LMMK that military service increases the likelihood of enrolling and completing a degree at a two-year institution and decreases the probability of enrolling and completing a degree at a four-year institution. This further bolsters the argument that we are likely to observe more completed educational attainment among service members separating after eight YOS in the degree data than in the enrollment data. If this hypothesis is correct, then the result that service members who separate after eight YOS are less likely than similar nonveterans to enroll in college over their lives may simply reflect the fact that we do not observe completed college enrollment over the 11-year time horizon where we observe college enrollment. On the other hand, this could simply be due to differences in college-enrollment and degree-attainment patterns across the cohorts for which we have enrollment and degree data. Unfortunately, we were unable to distinguish between these two hypotheses with the data currently available to us.

Figure 4.4
Estimated Effect of Enlistment on College Degree Attainment, by Years Since Application and Years of Service Prior to Separation, 1998–2000 Application Cohorts



Results by Military Occupational Specialty

Figures 4.5 and 4.6 show estimated effects of military enlistment by a service member's first MOS for four different occupational groups. Results for the complete set of one-digit MOS groups are shown in Tables A.14–A.17 in the appendix. As with the analogous results for earnings, these results pool across YOS. Thus, the estimates should be thought of as the difference between the earnings of veterans who go into a particular MOS and those of comparable nonveterans, where some of the veterans are still serving in active duty and others have exited.

The results suggest there is very little difference in college-enrollment and degree-attainment patterns by MOS. The one exception to this general rule is service members who enter the military in the health care field. These service members are significantly more likely than service members who enter into other occupations to enroll in and complete college over their lives. They also do so earlier in life. For example, service members in health care occupations are just 7.5 percentage points less likely than similar nonveterans to have enrolled in college during the second postapplication year, a time when service members in other occupations are closer to 10 percentage points less likely than similar nonveterans to have done so. Service members in health care occupations begin to catch up with similar nonveterans in terms of college-enrollment rates starting in the second year, while service members in other occupations do not begin to do so until the third or fourth postapplication year. Finally, although service members who enter in a health care occupation are 7.5 percentage points more likely than similar nonveterans to enroll in college by their 11th postapplication year, service members who enter into other occupations are only about as likely as similar nonveterans to do so.

There are several possible explanations for these patterns. First, service members who enter the service in health care occupations may exit military service at different times on average, relative to other service members. However, this explanation seems unlikely given that the earnings patterns of service members who enter in health care occupations do not differ systematically from those of service members entering in other occupations. Perhaps a more likely explanation is that service members who enter in health care occupations generally transition to civilian careers that require college credentials, such as nursing and other medical professions. Thus these service members are more likely than others to utilize their educational benefits and obtain higher education during or after separation from military service. They may also do so sooner after military service, on average, because they are unable to fully transition to the civilian labor market in their desired professions until they complete college.

Figure 4.5
Estimated Effect of Enlistment on Cumulative College Enrollment, by Years Since Application and Military Occupational Specialty, 1998–2000 Application Cohorts

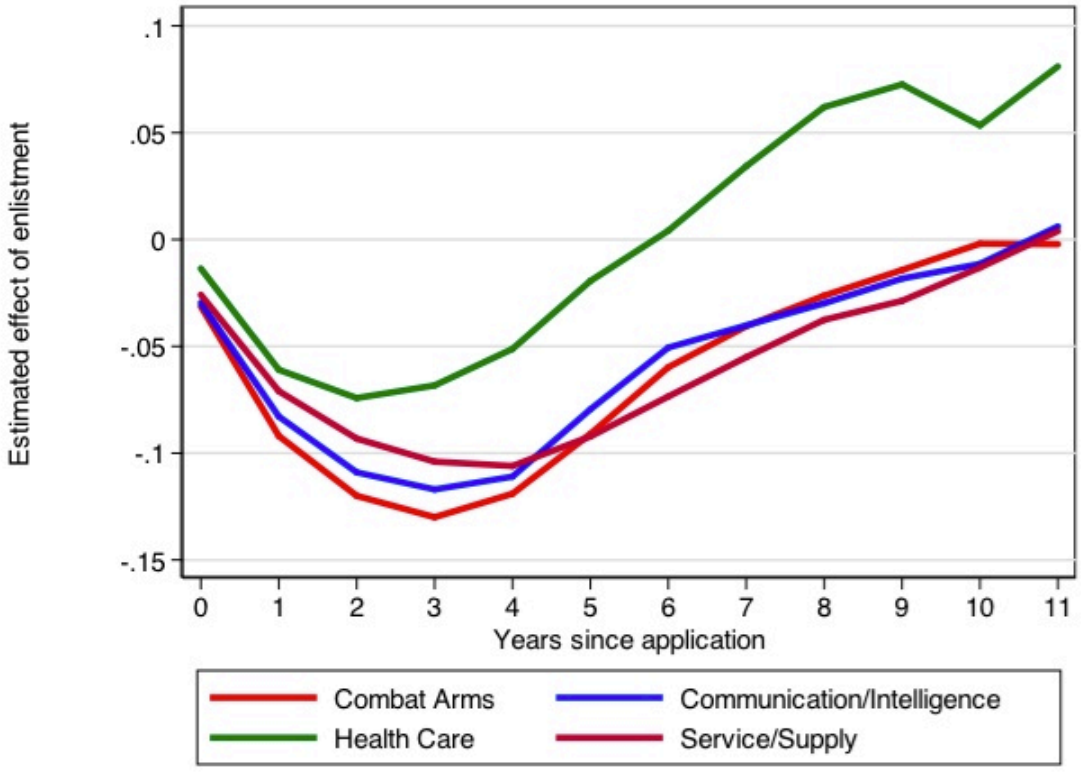
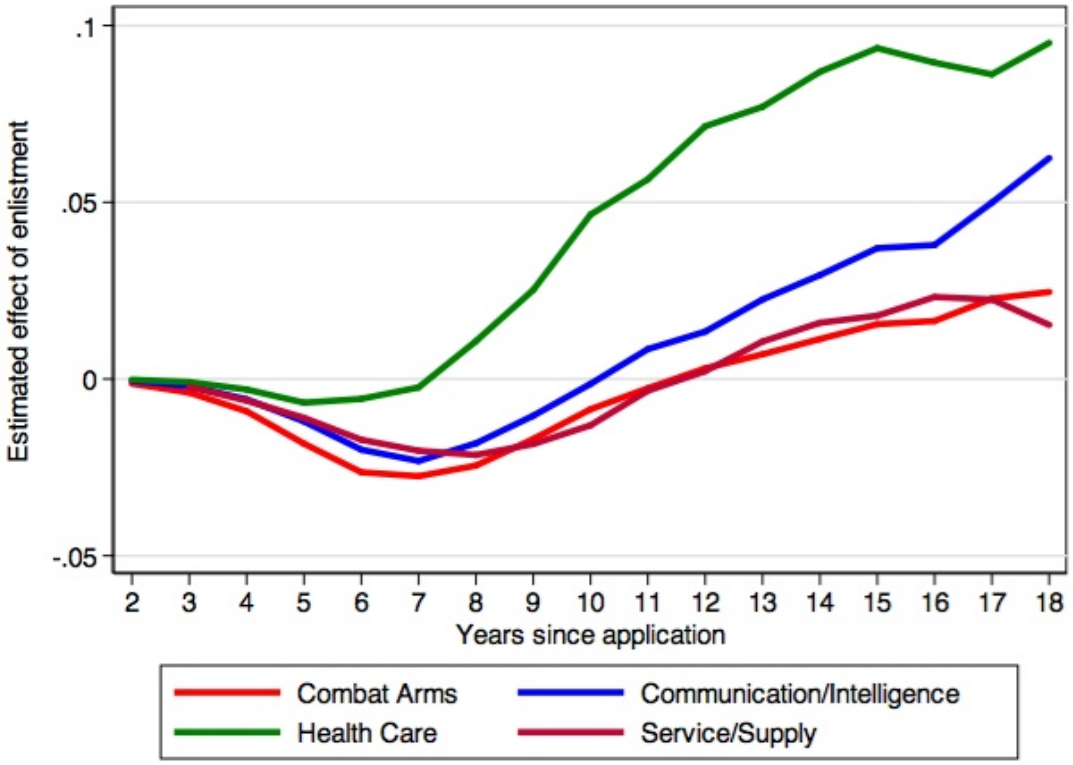


Figure 4.6
Estimated Effect of Enlistment on College Degree Attainment, by Years Since Application and Military Occupational Specialty, 1991–1994 Application Cohorts



In summary, joining the military is associated with significantly delayed college enrollment, but service members are equally likely to enroll in college at some point during their lives as similar nonenlistees. In fact, if we could observe college enrollment outcomes later in life, we might find that service members eventually enroll at rates that are statistically higher than those of nonenlistees.

Service members who serve five to seven years and then separate from military service may be the most likely to use their education benefits and enroll in college, and they do so soon after separating. Those who separate after four or fewer years delay their college enrollment, but they do eventually catch up with their civilian counterparts.

We also find very little difference in college-enrollment and degree-attainment patterns by MOS, except that service members who enter the military in the health care field are significantly more likely to enroll in and complete college and to do so earlier in life.

Finally, it is important to note that the findings in this report mainly focus on an era when veterans were eligible for the benefits granted in earlier GI Bills. The passage of the much more generous Post-9/11 GI Bill means that military service might have different effects on

educational attainment in the future. We suggest that more research be conducted on the effect of recent changes to veterans' educational benefits.

Chapter Five. Analysis of the Effect of Economic Conditions at the Time of Separation

The results in Chapter Four indicate that veterans who complete two or more YOS have considerably higher long-run earnings than do nonveterans with similar characteristics at the time of application. However, these earnings differences may be affected by external factors and policies, and it may be possible to increase these returns. In an effort to better understand how external factors and policies affect these impacts, in this chapter, we examine how economic conditions in the civilian labor market at the time individuals exit active duty affect postservice earnings.

At enlistment, military service members commit themselves to an initial term of service, during which time there are ex ante predictable and unpredictable (to the service member and to the military) developments in the economy. We isolate the unpredictable component of economic conditions at the end of the first contract in order to study their effect on reenlistment, college going, the earnings dip, and future earnings of recently discharged veterans. Here, we report the effect of changes in veterans' home-state unemployment rates on earnings, distinguishing between the effect on all service members and the effect on those who do not reenlist. To do this, we used quasi-random variation in economic conditions at the end of service members' first-term contracts to assess the impact of economic conditions at the time of separation.

Data and Sample

Our sample consists of the universe of military enlistees who entered between 1991 and 2006 as E1s, the lowest administrative rank in the military. To focus on the relevant population, we dropped those who did not complete the first two years of their initial contracts; the percentage of dropouts is unrelated to unemployment. This leaves just over 1 million service members, 1.06 million men and 160,000 women. Of these, 39 percent will reenlist, with this percentage increasing by 1 percent for each 1-percent increase in home-state unemployment. This is a robust result in all studies of reenlistment; for example, see Asch, Heaton, et al. (2010) for a discussion of the effects of unemployment rates on reenlistment rates and quality in this era. For most of our analysis, we focus on the results for men.

We constructed service histories—in particular, month of enlistment and initial contract length—using information from the Proxy PERSTEMPO file that codes the months remaining on the current contract. *Reenlistment* has no formal definition; we adopted the usual empirical definition in the military manpower literature and considered increases in remaining months of military service of 24 months or more as reenlistment events (Hosek and Martorell, 2009). We

classified a service member as having reenlisted if a reenlistment event occurred before the end of his or her first contract. Service members may pursue other avenues to remain in the military, including shorter extensions to their contracts. In principle, service members could extend their initial contracts beyond the initial expiration and reenlist in this later period and still be missed by our definition of *reenlistment*. As well, a service member may exit the service and return at a later date. We find evidence that these less common avenues of extending military service play a small role past the year in which the initial contract ends.

State unemployment rates come from the Bureau of Labor Statistics Local Area Unemployment series. We defined the service member's home state as the state in which the service member enlisted as this is recorded in the military administrative records. We did not observe migration, either in the military or after separation. We suspect that significant measurement error is introduced here, both in the Bureau of Labor Statistics series (because of sampling variation) and the use of state of enlistment as a proxy for state of post-discharge employment. If we wanted to interpret our results as the causal effect of the relevant unemployment rates at discharge, this measurement problem would bias our results toward zero.

Methods

Our empirical strategy uses cross-state variation in unemployment rates, holding constant national conditions, average state differences, and other observable characteristics. For example, we compared the earnings of service members eligible for discharge in 2005 from North Dakota with earnings of those eligible for discharge in 2005 in Nevada, accounting for the average earnings difference between North Dakota and Nevada, and other individual observable predictors of different earnings (e.g., race/ethnicity, AFQT); the difference we attributed to the unemployment rate. Controlling for national conditions at discharge allowed us to use the entire sample period without explicitly accounting for the conditions service members encountered while in the military or for the various reenlistment regimes over this era. Results using national unemployment rates in the same framework produce larger, more-persistent earnings losses. This could reflect the slope of the military's labor demand curve (the military may reduce reenlistment incentives during times of high national unemployment rates) or a spurious correlation between military labor demand and national business cycles. Because the economics literature posits many links between military spending and the business cycle, we prioritized the analysis based on state-level variation.

We report two empirical models of earnings responses to unemployment. The first empirical model estimates the intention-to-treat (ITT) (Angrist and Imbens, 1995) effect of unemployment on the three earnings types. This estimate reflects the total impact of the home-state unemployment rate at the end of the service member's first contract on his or her future earnings. Because reenlistment insures service members against earnings losses, the ITT estimates will exhibit an increase in military earnings and a decrease in civilian earnings.

The ITT model is as follows:

$$Earnings_{iostjk} = \gamma_{0k} + \gamma_{1k} UR_{st} + \gamma_{ok} + \gamma_{sk} + \gamma_{tjk} + \mathrm{X}_i' \prod_k + \zeta_{iostjk}.$$

Here, $Earnings_{iostk}$ is the military, civilian, or total earnings of individual i in military occupation o with state of enlistment s; eligible for discharge in year t; enlisted in period $t - j$; and observed in period $t + k$. Thus, k indexes time since the end of the first contract. The γ_{ok}, γ_{sk}, and γ_{tjk} coefficients capture means within occupation, state, and year of discharge eligibility crossed with year of entry. X_i contains time-invariant background demographic and intake characteristics and a large number of variables to reduce concerns of confounding factors biasing the estimates. Besides interactions of quadratics in age at eligibility with AFQT at entry, education at enlistment, family status at enlistment, race/ethnicity, and a set of physical measurements from intake, X_i contains the unemployment rate at the time of enlistment. The combination of these control variables captures any nationwide effects associated with the combination of year of enlistment and year of eligibility, as well as state- and time-specific information with the unemployment rate at enlistment. We estimated the model separately for each potential experience level k, where UR_{st} remains fixed but the parameters (the γs and Π) vary by k (i.e., years since end of initial contract). This imposes no structure on the time pattern of earnings losses or the covariates' contribution to explaining these losses.

Estimates in this model reflect several assumptions; where discretion enters, we chose conservative methods that may underestimate losses. Identification depends on the assumption that UR_{st} is unpredictable at enlistment, conditional on the variables detailed above. Put simply, this means that, once we control for the national business cycle at entry and exit, plus the unemployment rate in home state at entry and the average state conditions, the realized state unemployment rate is effectively random. The structure of the model assumes a time-invariant, linear relationship between state unemployment rates and earnings that is the same across all states. We find no role for higher-order terms in our analysis but cannot rule out non-linearities when extrapolating our estimates to the out-of-sample unemployment rates currently facing discharged service members. As well, we assumed that fixed effects in state, year, and occupation capture stable means in these dimensions. Violations of this assumption would threaten our strategy only as the omitted trends correlate with unemployment rates at discharge.

In the second empirical model, we restricted the sample to those who exit and estimated a two-stage selection model of the reenlistment decision and earnings outcome. The selection correction is a standard implementation of the two-step Heckman selection correction, in which a selection probability estimated in the first-stage model is entered as a control variable in the main equation (Heckman, 1979). We used a transformation of the selection probability to maximize its potential explanatory power (Newey, 2009). This average treatment effect (ATE) estimate reflects the causal impact of the unemployment rate on those who exit the military, accounting for the compositional change in this group and the insurance against idiosyncratic shocks provided by reenlistment. This model tells us the impact of the unemployment rate on the future

earnings of those induced to separate from the military (for the average marginal reenlister over the sample period), an important event as the military draws down during the recession.

The selection-corrected ATE model is as follows:

$$Earnings_{iostjk} = \beta_{0k} + \beta_{1k} UR_{st} + \beta_{ok} + \beta_{sk} + \beta_{tjk} + \mathbf{X}_i' \Theta_k + \Sigma_m \lambda_{mk} p_{iost}^m + \eta_{iostjk},$$

which has the same structure as above, with the addition of the selection correction. The variable p is a transformation of the probability of reenlistment within occupation for the sample of non-reenlisters; we used a polynomial of degree 4 throughout. We restrict the sample in this model to those not reenlisting.

The first-stage selection equation is estimated once for the period in which the initial contract ends:

$$P\left(Reenlist\left(Z_{iost}\right)\right) = \alpha_0 + \alpha_{ot} + \alpha_s + \alpha_{tj} + \alpha UR_{st} + \mathbf{X}_i' \Omega + \varepsilon_{iost}.$$

Time variation in the probability of reenlistment within occupation, α_{ot}, identifies this selection equation, under the assumption that occupation-specific reenlistment rates are uncorrelated with state unemployment rates, once we control for national trends. In other words, once we control for national conditions in period *t*, occupation-level reenlistment rates are unrelated to state-level unemployment rates. Under this assumption, the percentage of an occupation who reenlist (captured by the α_{ot} fixed effect) tells us the degree to which the distribution of exiters has been selected and can be excluded from the civilian earnings equation because it does not affect the potential earnings of those who exit. Entering controls for this selection term allows us to interpret our results as the causal effect of separating for those induced to separate by the military's reenlistment policies and incentives. We do not model these policies and incentives but rather take the outcome (at the national level) as indicative of the selection pressures on exiters.

In addition to the earnings losses, a statistic of interest is

$$\Delta = 1 - \frac{\Sigma_k \delta^k \gamma_{1k}}{\Sigma_k \delta^k \beta_{1k}}$$

for total earnings, where the ratio of sums reflects the average losses of service members regardless of reenlistment behavior relative to the additional losses among those forced to exit. Put simply, this statistic is the portion of the losses that has been insured by reenlistment over this era. In our data, we report the present discounted value of losses for a ten-year window following eligibility for discharge.

The estimation of these models deserves some comment. To access the SSA earnings records, we sent programs via email that were run on secure computers at the SSA. These programs performed a preliminary regression analysis at the individual level, returning to us the average residual within, e.g., state by quarter of discharge eligibility by sex cells. We conducted our analysis on these residuals. The analysis at the branch or reenlistment level conditions these cells further on these characteristics (e.g., state by quarter by sex by branch). To prevent the recovery of individually identifiable earnings outside SSA, cells with fewer than five

observations were dropped in the analysis data set. This procedure had no effect on the estimates for men (less than 1 percent of observations were dropped in any specification); however, results for women may be compromised (drop rates are 10 percent in the main specification and much higher with further division of the cells). It is difficult to quantify the competing effects of a higher variance in the women's earnings and the noise introduced by the procedure. Another concern that we cannot address here relates to the proper adjustment of the standard errors in the ATE model. Because we did not jointly estimate the selection model and main earnings equations, we cannot adjust for the covariance structure between these models (Murphy and Topel, 1985). The selection model does not change our estimates in any meaningful way, so this seems unlikely to bias our results. We plan to address these issues by estimating the entire model at SSA in future work.

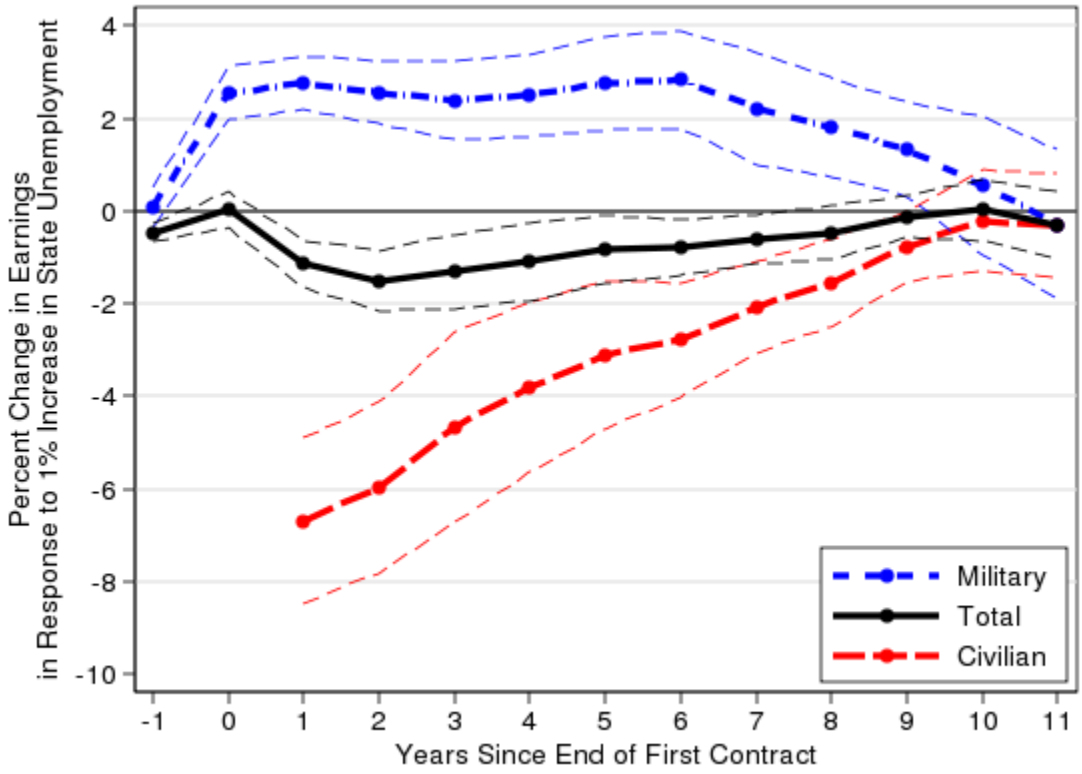
Several important issues remain unaddressed by the empirical methodology employed here. First, our analysis restricts attention to earnings; obviously, earnings do not represent overall welfare. Reenlistment insures service members against unemployment shocks through the substitution of years spent in civilian life for years spent in the military, a cost we do not account for here. A description of the underlying choice problem in terms of utility would allow us to evaluate the consequences of these shocks on the overall welfare of service members. Second, our current results do not incorporate GI Bill usage. In a separate analysis, similar to Simon, Negrusa, and Warner (2010), we find small effects on enrollment and total benefit usage in college benefit programs as a response to home-state unemployment shocks; however, we expect the inclusion of college benefit usage in our analysis to reduce the size of earnings losses. Because we lacked data on state of residence following discharge, we used state of enlistment to assign unemployment rates; although the analysis would benefit from these additional data, given the choice, we should prefer state of enlistment over state of future residence because state of residence will be endogenously chosen in response to unemployment rates. Finally, we restricted our analysis to the first reenlistment point for enlisted service members, setting aside dynamic effects. A model of the dynamic choice problem is likely required to assess the efficiency costs of this insurance mechanism.

Results

We report results for the complete (unbalanced) sample of those whose contracts ended between 1993 and 2010; we are not conditioning on reenlistment yet. Results for a balanced sample of cohorts eligible for discharge in the 1990s show the same patterns with slightly larger standard errors. Standard errors are clustered by state with 95-percent confidence intervals displayed in dashed lines on our figures. Figure 5.1 shows the ITT estimates of the effect of a 1-percent increase in unemployment on the military, civilian, and total income of male service members. Year 0 is the year in which the first contract ends. These estimates reflect the impact of a 1-percent increase in state of enlistment unemployment on the subsequent earnings of all members of our sample. Substitution from civilian income to military income reflects both

reenlistment and other avenues of extending military service. This change in the composition of income persists for nine years but does not offset the loss in civilian income until after the seventh year. As a result, mean total income falls, with peak losses approaching 2 percent in year 2.

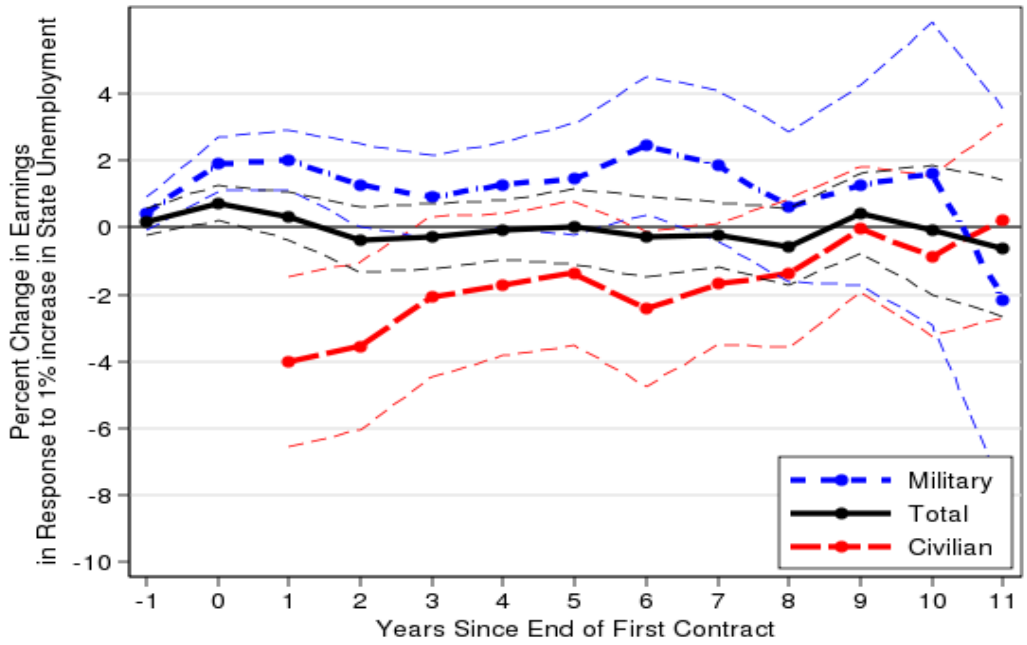
Figure 5.1
Estimated Effect of Home-State Unemployment on Annual Earnings, by Years Since End of Initial Contract, Men, 1991–2006 Entry Cohorts



NOTE: Thin dashed lines denote 95-percent confidence intervals. Years –1 and 0 have low civilian base earnings and are omitted to maintain scale; estimates appear in the appendix.

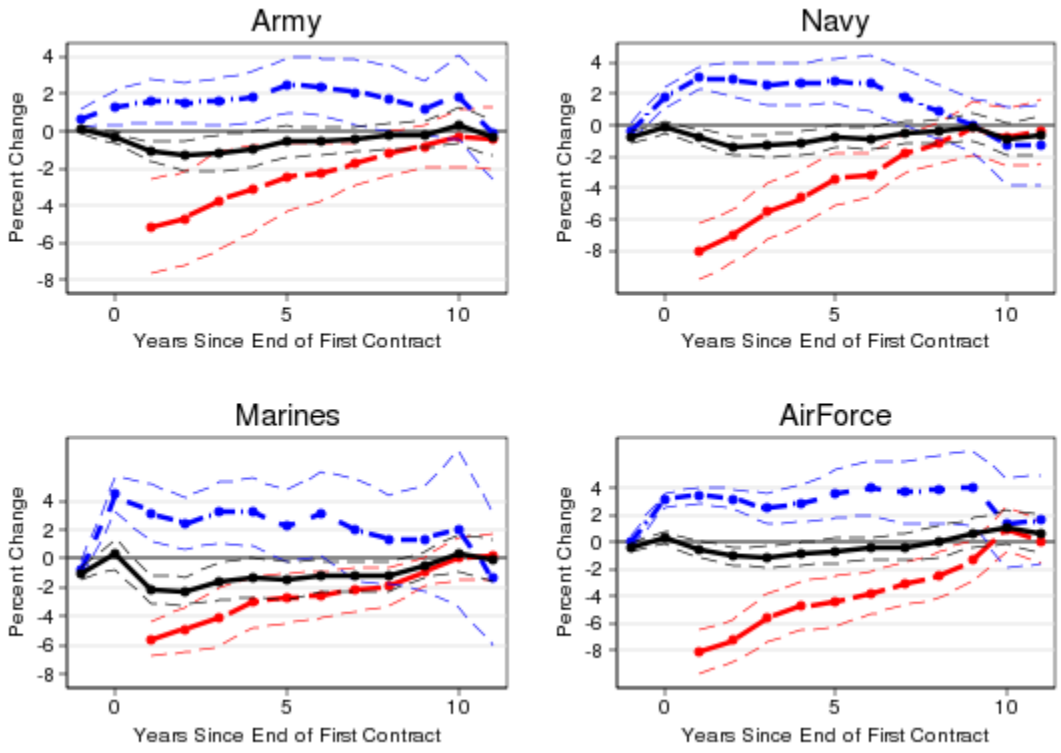
The results for men contrast with those for women, depicted in Figure 5.2. Results on the composition of income resemble the results for men, with clear substitution of military earnings for civilian earnings; however, estimated effects are much more variable and statistically less persistent. Women's total earnings actually rise in the year of discharge in response to an increase in unemployment, a result consistent with income and secondary-earner effects. Unfortunately, we lack the statistical power to explore these results in more detail. Subject to the caveats above, what evidence we have suggests that different forces may determine the responses of male and female earnings to unemployment shocks. We focus on men in the remaining analysis.

Figure 5.2
Estimated Effect of Home-State Unemployment on Annual Earnings, by Years Since End of Initial Contract, Women, 1991–2006 Entry Cohorts



NOTE: Thin dashed lines denote 95-percent confidence intervals. Years –1 and 0 have low civilian base earnings and are omitted to maintain scale; estimates appear in the appendix.

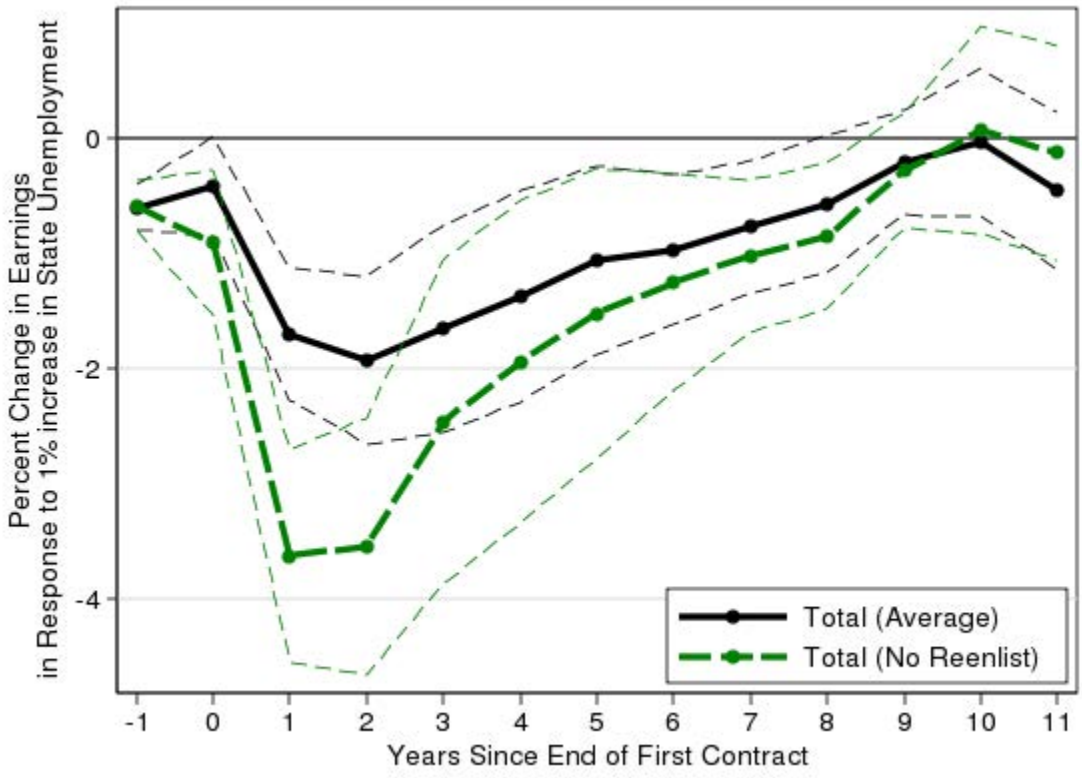
Figure 5.3
Estimated Effect of Home-State Unemployment on Annual Earnings, by Years Since End of Initial Contract, Men, 1991–2006 Entry Cohorts, by Branch of Service



NOTE: Thin dashed lines denote 95-percent confidence intervals. Years –1 and 0 have low civilian base earnings and are omitted to maintain scale; estimates appear in the appendix.

Figure 5.3 disaggregates results for men by branch of service. Broadly, these figures display the same patterns of substitution and earnings losses across the four branches. Patterns for the Navy and Air Force show larger substitution and more-precise estimates of the responses to unemployment shocks. In all four branches, unemployment shocks have persistent effects on earnings.

Figure 5.4
Estimated Effect of Home-State Unemployment on Annual Earnings, by Years Since End of Initial Contract, Men, 1991–2006 Entry Cohorts, by Reenlistment



NOTE: Thin dashed lines denote 95-percent confidence intervals.

Figure 5.4 reproduces the total earnings result from Figure 5.1 and adds the total earnings effect for those who do not reenlist before the end of their first contract. Total earnings for those not reenlisting have been corrected for selection into reenlistment, though this has virtually no impact on the estimates. These response patterns can be interpreted as the average response to unemployment over our sample period (in the presence of the "average reenlistment policy" over this period) and the effect of unemployment on those unable to reenlist. As the military draws down over the coming years, the average service member will be pushed from the average effect toward the no-reenlistment effect. As striking as the nearly 4-percent peak total earnings losses are among those unable to reenlist, the total earnings losses here are not as steep as the decline in civilian income in these years, suggesting that those who do not reenlist continue to draw some military income.

We can quantify the earnings insurance provided by the military by comparing the present discounted value of the two earnings effects in Figure 5.4. This calculation turns out to be relatively insensitive to the discount rate we use. Setting the discount factor δ equal to 0.95, 0.9, and 0.8 results in insured losses of 1.17 percent, 1.11 percent, and 1.07 percent, respectively,

over the ten years following the end of the first contract. For those unable to reenlist, losses are 1.98 percent, 1.80 percent, and 1.69 percent, implying that reenlistment insures between 36 percent and 41 percent of total earnings losses, a number consistent with the 39-percent average reenlistment rate. Of course, the price of this insurance is the substitution of years spent in civilian life for years in the military, a cost we do not account for here.

In summary, among enlistees, we find that both men and women substitute military income for civilian income when unemployment rises. Despite this substitution, men's total earnings decrease in response to high rates of unemployment at the end of their first contract, with significant effects on total income lasting seven years and on the composition of income for nine years. Women's total earnings are higher in the year of eligibility, but the effects are indistinguishable in the years following.

These effects are economically meaningful—our data do not yet contain earnings histories for those separating in the Great Recession, but unemployment rates in many states exceeded their historic levels by more than 5 percent during the peak of the recession and remain high in 2013. Given our estimates that suggest that the peak earnings losses associated with a 1-percentage-point increase in the unemployment rate are about 4 percent of mean earnings, the unusually high unemployment during the Great Recession may have led to earnings losses of 20 percent in some areas where the unemployment rate increases were especially large.

These results have several implications for military personnel policy. According to our findings, government expenditures on continued employment have important multiplier effects on earnings. Continued employment shields workers from the persistent effects of separation in times of high unemployment. Service members place a high value on earnings during recessions, not only because they reduce the magnitude of economic shocks but also because these shocks have persistent effects. It appears that the military could offer an insurance policy that would allow a reduction in military wages and an increase in the welfare of service members, providing service members with direct insurance against these shocks, rather than the indirect protection offered by reenlistment. Increases in reenlistment during recessions may have negative consequences because those induced to reenlist by unemployment will assign the lowest value to continued service. This may result in a shift in the quality of reenlisters, lower retention at future reenlistment points, and crowding in certain ranks and occupations.

Chapter Six. Analysis of the Effect of the Army Partnership for Youth Success Program

The results presented thus far suggest that military service may have sizable returns in the form of higher earnings but that external factors, such as economic conditions at the time of military separation, can have important effects on postservice earnings. Given the persistent weakness in the U.S. labor market, it is therefore important to understand whether there are policies that can make the transition to civilian employment smoother.

An example of such a policy, the PaYS program, is a partnership between the U.S. Army and U.S. employers that allows the Army to offer postservice employment opportunities as a recruitment incentive. The Army signs memoranda of agreement (MOAs) with employers that are interested in employing service members upon separation. Eligible employers must have a minimum of 500 employees and be willing to offer full-time positions to separating service members. The PaYS website lists 400 participating public- and private-sector employers, including the New York City Police Department, the City of Chicago, Walgreens, Six Flags, and Amazon.com. Each PaYS employer is expected to project future openings for separating service members by MOS and list those openings in an Army database. Although participating employers commit to interviewing PaYS participants for those positions, they are not obligated to hire them.

The recruitment incentive is available only to individuals who have completed a high school diploma (tier 1) and achieve a score of 31 or higher on the AFQT (categories 1–3B). The PaYS program is offered to both enlisted soldiers and reserve-component Reserve Officer Training Corps (ROTC) cadets; however, this project focuses on enlisted PaYS participants. According to discussions we had with subject-matter experts, the PaYS incentive is offered to all individuals who qualify, and those individuals do not lose access to any other enlistment incentives by choosing to participate in the program. During enlistment processing, applicants select their MOS and associated incentives and terms of enlistment. This provides enlistees with a menu of PaYS partners that are projected to have positions available at the completion of the contract. The enlistee chooses a potential employer and is promised an interview upon separation, as long as the service member receives an honorable discharge and is otherwise qualified for the position. For the soldier, the recruitment incentive offer is attractive because it allows a "foot in the door" with a large employer upon separation and an opportunity to use the skills learned in the Army to transition to civilian employment.

Although the primary function of PaYS is to serve as an Army recruitment tool, the program could offer benefits to service members, including the prospect of a job that is aligned with the skills they develop in the military and a smoother transition into the civilian labor market with no

or less of an earnings drop at separation. However, for the PaYS program to have a positive impact on labor market outcomes, the following must all be true:

- The Army and the PaYS partner must ensure that the individual receives an interview with the employer upon separation.
- The former service member must follow through with the interview and present him- or herself as a reasonable candidate.
- The PaYS employer must hire the former service member.
- The PaYS employment must be superior to the employment that the former service member would have achieved without PaYS by providing a job sooner or providing a more desirable job (e.g., higher salary, better matched to interests).

This chapter explores the impact of the PaYS program on annual earnings after separation. We first describe the sample for the project and our methods for analysis, and then we discuss the results.

Sample

As described in Chapter Two, we used SSA data to measure SSA, civilian, and military earnings. We linked these earnings records to DMDC data on all enlistment contracts for all Army enlistees who joined between January 2001 and October 2004. These data also have basic information from the service member's MEPCOM and PERSTEMPO records, allowing us to identify separation dates for service members. We limited the sample to enlistees who had at least a high school degree and an AFQT score of 31 or higher because this is the target group for the PaYS program. The PaYS program was scaled to serve a substantial number of enlistees in April 2003, with the take-up rate among Army enlistees of approximately 11 percent. We limited our sample to enlistments prior to November 2004 to ensure that we had earnings data available beyond the end of the contract period.

In Table 6.1, we present summary statistics on the individuals who enlisted during the time period where PaYS was offered, April 2003 to October 2004. Over the time period we examined, PaYS served more than 6,600 individuals, approximately 11 percent of all enlistment contracts signed during that period. PaYS participants are more likely than other enlistees to be male, more likely to be white or Hispanic, and less likely to have education beyond a high school degree. PaYS participants are also younger and more likely to be single, less likely to have kids, and have slightly lower AFQT scores than those who did not choose to take the PaYS enlistment incentive.

Table 6.1
Summary Statistics for Contracts in April 2003–October 2004, Individuals Who Qualified for the Partnership for Youth Success

Characteristic	No PaYS (N = 53,899)	PaYS (N = 6,625)	Difference
Female	0.204	0.166	0.038***
White	0.629	0.646	–0.017***
Black	0.168	0.158	0.01**
Hispanic	0.138	0.147	–0.009***
Other race or ethnicity	0.064	0.048	0.016***
High school graduate	0.831	0.943	–0.112***
Some college	0.106	0.046	0.060***
Bachelor's degree	0.056	0.005	0.051***
Graduate education	0.004	0	0.004***
Single	0.834	0.86	–0.026***
Married	0.149	0.123	0.026***
Divorced or other	0.017	0.017	0
Has dependents	0.178	0.149	0.029***
Age	21.16	20.45	0.710***
AFQT	59.42	57.04	2.38***

NOTE: *** = $p < 0.01$. ** = $p < 0.05$.

Methods

As the discussion above demonstrates, not all service members elect to take the PaYS program at the time of enlistment, and the baseline characteristics of PaYS participants differ from those of nonparticipants. There are a variety of ways that selection into PaYS participation may be related to subsequent earnings. On the one hand, Army recruiters may explicitly offer the program to only very high-quality recruits in whom they are highly interested in recruiting. In this case, we would expect simple means comparisons of labor market outcomes of PaYS participants to be upwardly biased estimates of the impact of PaYS, because PaYS participation is a proxy for high quality. On the other hand, recruiters may explicitly offer the program only to service members who are wavering about whether to enlist or not, and the direction of bias in this case is unclear and depends on whether "wavering recruits" are more or less likely to have better labor market outcomes. Finally, PaYS may be of interest only to recruits who expect to have poor labor market prospects or difficulty getting good interviews. In this case, we would expect simple means comparisons to underestimate the true impact of PaYS because PaYS take-up is a proxy for poor labor market expectations.

To mitigate the concerns outlined above, we controlled for the baseline characteristics included in the PaYS data. Specifically, we ran models of the following form using ordinary least squares (OLS):

$$E_{it} = a_t + B_t PaYS_i + X_i G_t + e_{it},$$

where E_{it} is an earnings measure (total, military, or civilian earnings) for service member i in the tth year following enlistment; $PaYS_i$ is an indicator for whether the service member is a PaYS participant; X_i is a vector of service member characteristics, including gender, race/ethnicity, education, marital status, number of children, age, and AFQT score; and e_{it} is an idiosyncratic, normally distributed error term.

The coefficient of interest is B_t, the impact of PaYS on earnings in year t. However, to interpret these estimates as causal, we must assume that, conditional on service member characteristics X_i,[21] PaYS take-up is unrelated to the error term e_{it}. We do not believe this to be the case in general. For example, if we believe that service members who expect to have poor labor market prospects are the most likely to take up PaYS, and poor labor market expectations is not fully captured by the vector X, then B_t is likely to biased downward. Nevertheless, the OLS approach helps to mitigate these concerns as much as possible.[22]

Results

As previously mentioned, we focused on total earnings, including military and civilian earnings. We ran this analysis separately for individuals with varying contract lengths: less than three years, three years, four years, and more than four years. Contracts of three years and four years are the most common, representing nearly 87 percent of all contracts. Our results for three- and four-year contracts were very similar, so we focus on presenting the results for enlistees with four-year contracts.

Figure 6.1 presents the raw difference in earnings for individuals who participate in PaYS and those who do not participate in PaYS. PaYS participants earn significantly less than non–PaYS participants, both before and after separation from the military. This difference is slightly more than $1,000 in the first year after enlistment, and the difference grows to more than $5,000 within seven years of enlistment. These lower earnings are driven by the fact that PaYS participants are younger than other enlistees, have lower aptitudes, and have lower levels of education, characteristics that are associated with lower earnings.

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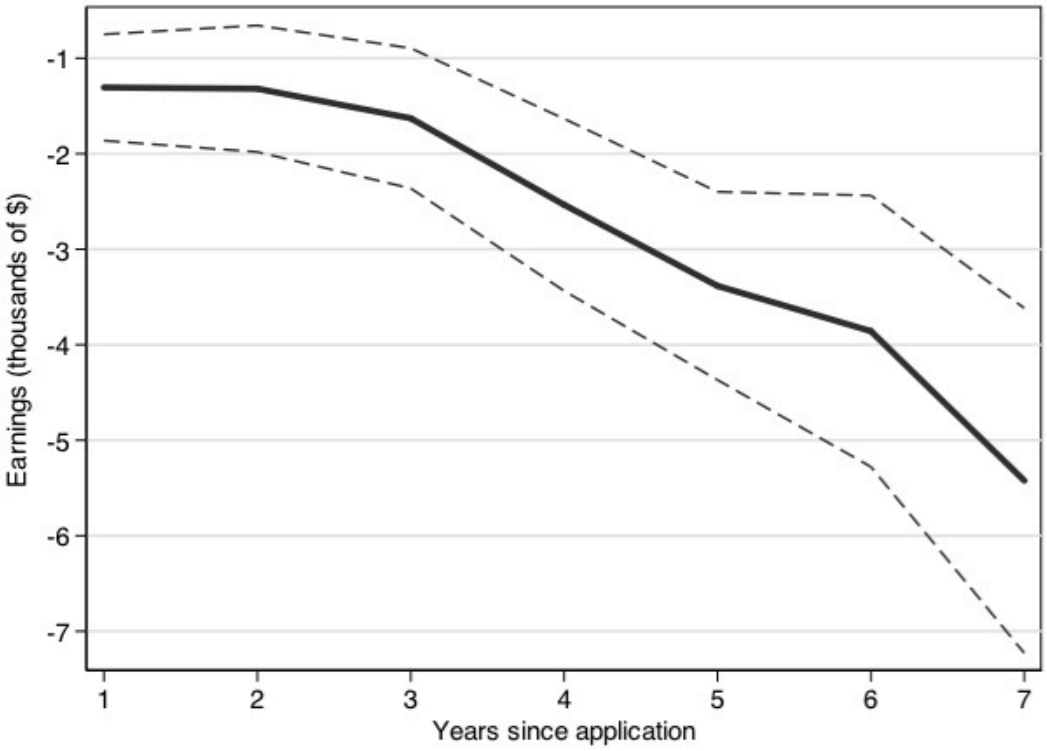
[21] X_i contains the service member characteristics listed in Table 6.1.

[22] Note that we explored the possibility of exploiting variance in PaYS take-up stemming from the precipitous increase in PaYS participation and marketing that occurred during April 2003. We experimented with instrumental variables models that used the months of April and May 2003 as instruments for PaYS uptake (results not reported). The identifying assumption is that there is nothing special about service members who enlisted during those two particular months but that enlisting during those months is related to PaYS participation. Unfortunately, the estimates from this approach were imprecise. We thus present the OLS estimates as our favored specification.

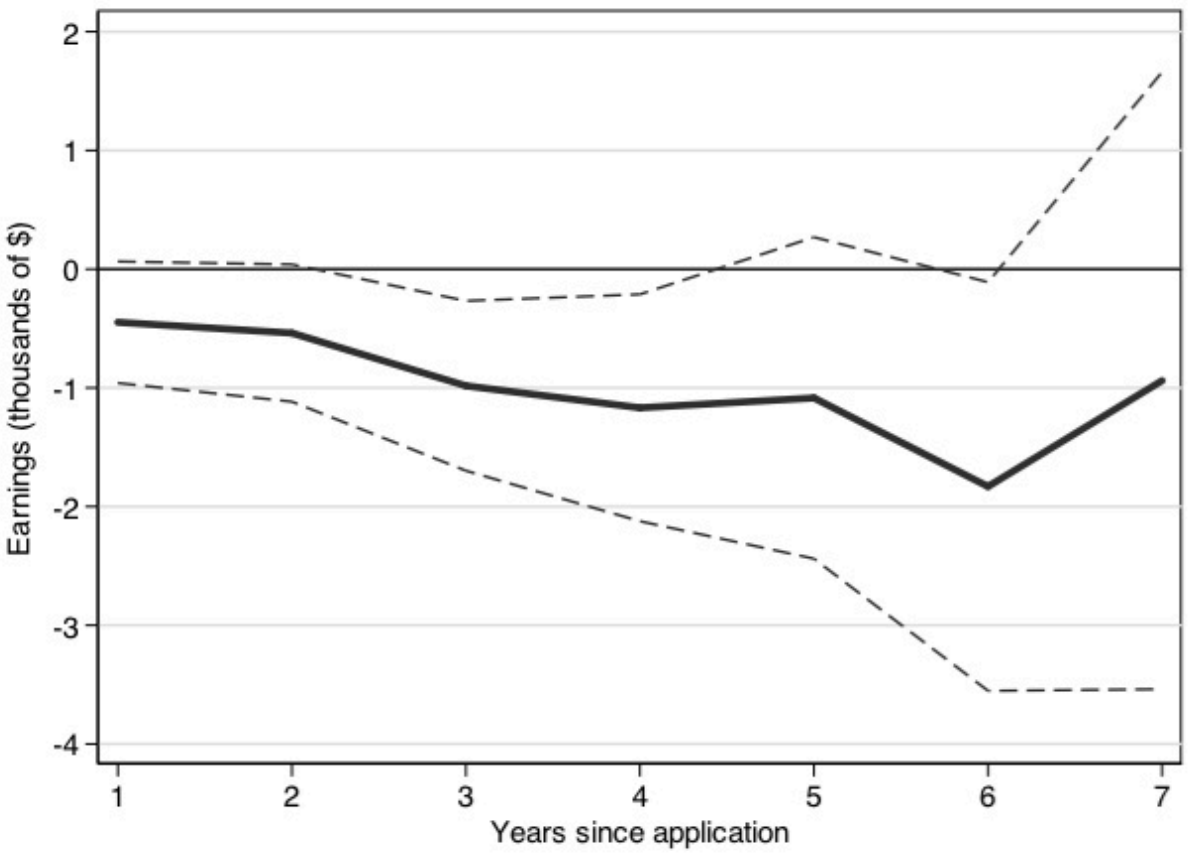
Figure 6.1
Raw Difference in Earnings, Partnership for Youth Success Participants Versus Nonparticipants



NOTE: Dashed lines denote 95-percent confidence intervals.

Figure 6.2 presents the estimates from our OLS analysis, which controls for differences in observable baseline characteristics of PaYS participants and nonparticipants. When we control for these characteristics, the differences in earnings between PaYS participants and other enlistees in the first year of enlistment are smaller and no longer significant. We have statistically significant estimates in years 3, 4, and 6, with PaYS participants earning approximately $1,000 less than comparable individuals who did not participate in PaYS in years 3 and 4 and nearly $2,000 less in year 6. The fact that we found significant results for year 3 suggests that there are some unobservable characteristics that we did not sufficiently control for, because there should be no differences in earnings until service members complete their contract terms and seek employment in the civilian labor market (year 4 and beyond).

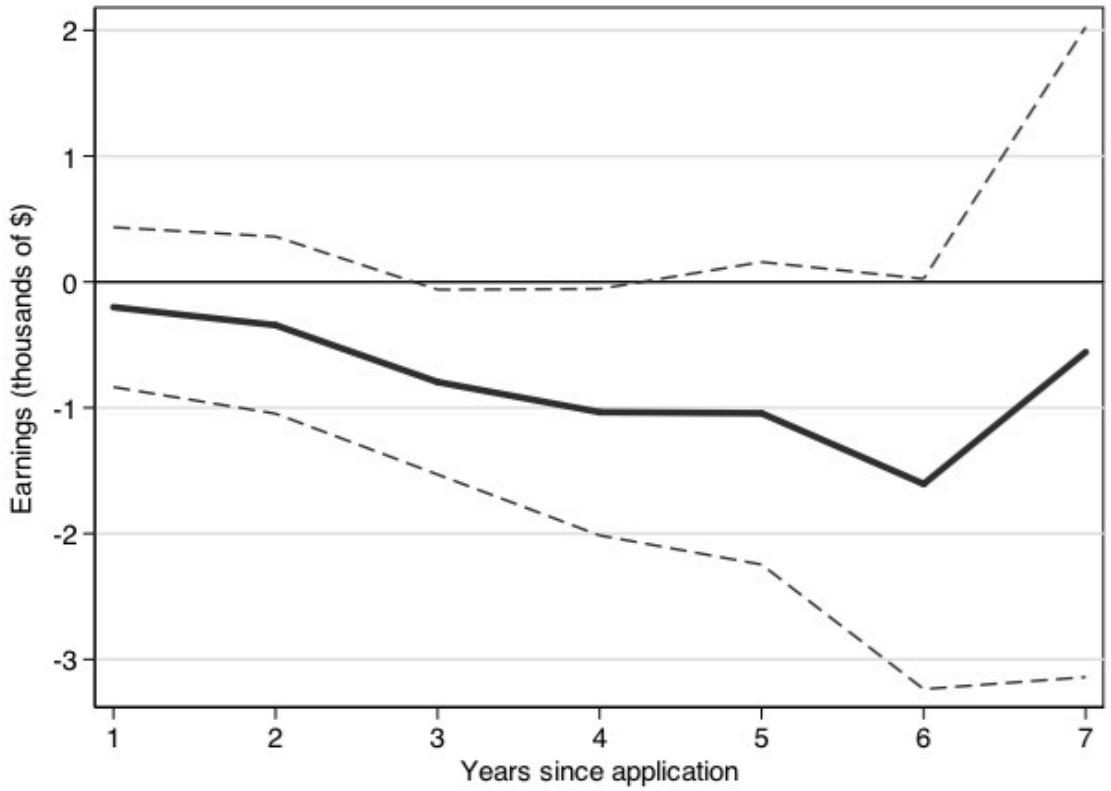
Figure 6.2
Impact of the Partnership for Youth Success on Annual Earnings, by Year Since Enlistment



NOTE: Dashed lines denote 95-percent confidence intervals.

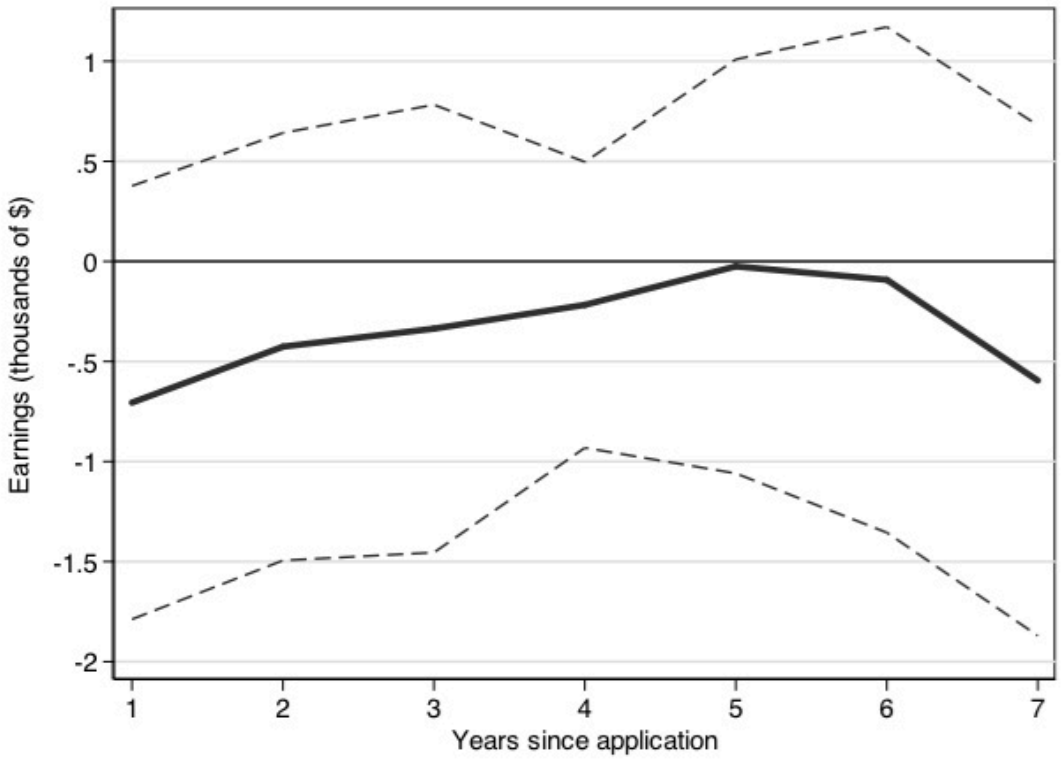
Figures 6.3 and 6.4 present estimates for the impact of PaYS on civilian and military earnings, respectively. Because selection into the military or civilian sector is likely to be related to participation in PaYS, these estimates should not be interpreted as causal effects. The PaYS participants who choose to remain in the military may be distinctly different from nonparticipants. For civilian earnings, the earnings most likely to be affected by PaYS participation, we find a small negative relationship with PaYS participation in years 3 and 4 and no significant relationship in other years. As expected, there is no significant relationship between PaYS and military earnings in any year.

Figure 6.3
Impact of the Partnership for Youth Success on Annual Civilian Earnings, by Year Since Enlistment



NOTE: Dashed lines denote 95-percent confidence intervals.

Figure 6.4
Impact of the Partnership for Youth Success on Annual Military Earnings, by Year Since Enlistment



NOTE: Dashed lines denote 95-percent confidence intervals.

Our analysis suggests that PaYS participation is associated with decreased earnings in years 3, 4, and 6, with no difference in earnings for other years. We find no relationship between PaYS participation and military earnings, so we know that these lower earnings are driven by earnings from civilian employment. Absent selection issues, we would have expected to see positive impacts on earnings from a program that, like PaYS, provides job searchers with a foot in the door with a good employer, so these results run counter to expectations.

We cannot eliminate the possibility that the negative estimates are driven by unobserved differences between PaYS participants and non–PaYS participants. The summary statistics and the raw differences in earnings presented in Figure 6.1 indicate that the individuals who choose to participate in PaYS have observable characteristics that are associated with lower earnings, such as education and AFQT scores. If PaYS participation is associated with other factors that drive earnings and we are unable to control for, such as an individual's personality, work habits, or motivation, it could be these unobserved characteristics that are driving our negative estimates.

As we explained in our introduction of the PaYS program, there are a series of factors that would need to occur for PaYS to result in increased earnings. There are many points at which the process might break down, resulting in no improvement in employment outcomes. Anecdotally, the Army reports that individuals are following through with interviews and, in some cases, receiving the job with the PaYS partner employer. It may be that these cases are not prevalent, or it may be that the opportunities individuals receive with PaYS employers are no better than what would have been obtained without PaYS. To better understand where the process might be breaking down, we would need to analyze data on whether employers follow through with granting interviews, whether former service members show up for interviews and make a good impression, and whether the PaYS participants are hired by PaYS employers. We would also want to conduct more-detailed analysis of the types of employers and jobs that PaYS participants and non–PaYS participants end up with after separation.

In summary, PaYS participation is associated with decreased earnings in years 3, 4, and 6, with no difference in earnings for other years. We know that these lower earnings are driven by civilian employment because we found no relationship between PaYS participation and military earnings. Nevertheless, the PaYS program may have met all of its goals for enhancing recruitment by providing individuals with a broader menu of attractive recruitment incentives, despite the lack of long-term impact on earnings. Although the PaYS program itself appears to be ineffective at increasing earnings, the idea of linking employers and veterans seems like a good one. The U.S. Department of Defense (DoD) and the U.S. Department of Veterans Affairs (VA) should work together to find out what the needs of veterans are when they enter the civilian workforce and develop interventions to address those needs. More broadly, we suggest more analysis of the effectiveness of veterans' transition assistance programs, including those offered by the U.S. Department of Labor. A better understanding of these programs is important especially in light of the lack of evidence of beneficial effects of PaYS and the finding (Chapter Five) that veterans who exit in times of high unemployment have lower earnings.

Chapter Seven. Summary and Conclusions

The analyses reported here provide valuable information to DoD and policymakers regarding the impact of military service on the economic well-being of service members. This information is critical to maintaining a compensation system that offers competitive, attractive pay and benefits and developing programs that assist veterans in their transitions to civilian employment. The following summarizes our findings and makes suggestions for next steps DoD and policymakers might consider.

The Effect of Military Service on Earnings

For the pool of all applicants, our findings on earnings mirror the findings reported in LMMK. A strong earnings gain peaks in year 2 after enlistment and declines through year 6. Thereafter, it steadily increases, and, by year 16, military service is associated with an average positive effect of about 8 percent of mean earnings. For all YOS values, there is a large return to being in the military in the years of active-duty military service. This premium falls sharply upon separation, but, several years after separation, the estimated returns become positive again and trend upward with time. Finally, for all occupational categories, military service is associated with sizable long-run earnings gains. Veterans who enter occupations in health care or communications or intelligence have larger gains relative to comparable nonveterans than do veterans who go into combat arms. The earnings gains for veterans who go into service or supply occupations fall in between these two other groups.

The degree to which skills are transferable to the civilian labor market varies by occupation. For instance, veterans who learn lots of skills in, say, truck repair or communication technology might have skills that are very valuable to civilian employers. However, even though some occupations have transferable skills, civilian employers might not understand the military occupation labels. We suggest that a military credentialing program would help with this problem. A formal credential or certificate that described a service member's course of study and range of skills could help civilian employers understand the value that veterans can bring to the workplace.

The Effect of Military Service on Education

Joining the military causes service members to significantly delay their college enrollment, but they are equally likely to enroll in college at some point during their lives as similar nonenlistees. In fact, given the trend we observe, we believe that, if we could observe college enrollment outcomes later in life, we would find that service members eventually enroll at rates that are statistically higher than nonenlistees.

Service members who serve five to seven years and then separate from military service may be the most likely to use their education benefits and enroll in college, and they do so soon after separating. Those who separate after four or fewer years delay their college enrollment, but their enrollment rates do eventually catch up with those of their civilian counterparts.

There is very little difference in college-enrollment and degree-attainment patterns by MOS except that service members who enter the military in the health care field are significantly more likely to enroll in and complete college and to do so earlier in life than those who enter other MOS.

We suggest that more research be conducted on the effect of recent changes to veterans' educational benefits. The findings in this report focus mainly on an era when veterans were eligible for benefits provided by the earlier GI Bills, but the passage of the much more generous Post-9/11 GI Bill means that military service might have different effects on educational attainment in the future. More work needs to be done on this question.

Finally, other research suggests large differences in earnings by college quality. Consideration should be given to providing information to service members to help them select and apply for college and otherwise optimize the use of their education benefits.

The Effect of Economic Conditions on Earnings

When examining the effect of economic conditions at the time of separation, we find two primary results. First, veterans appear to substitute military income for civilian income when they separate in times of relatively high unemployment (perhaps by entering the reserves). Despite this substitution, men's total earnings decrease in response to high rates of unemployment at the end of their first contract. We find that these negative effects of the unemployment rate last up to seven years.

These effects are economically meaningful—our data do not yet contain earnings histories for those separating in the Great Recession, but unemployment rates in many states exceeded their historic levels by more than 5 percent during the peak of the recession and remain high in 2013. Given estimates of peak earnings loss associated with a 1-percentage-point increase in the unemployment rate of 4 percent of mean earnings, earnings losses in the Great Recession may be around 20 percent of earnings in states where the recession was especially bad. To be clear, though, this problem is not unique to military veterans. Many workers, including those leaving college or first entering the workforce, may have also suffered from large and persistent earnings losses due to the Great Recession. The point of our analysis is to provide some evidence on what these losses may have been for veterans and to quantify the "insurance" value provided by military employment during economic recessions.

The Effect of the Partnership for Youth Success Program on Earnings

PaYS participation is associated with decreased earnings in years 3, 4, and 6, with no difference in earnings for other years. Because we found no relationship between PaYS participation and military earnings, we can conclude that these lower earnings are driven by civilian employment.

The PaYS program may very well have met all of its goals for enhancing recruitment by providing individuals with a broader menu of attractive recruitment incentives, despite the lack of long-term impact on earnings. However, there is a broader policy interest in the impact of employer-employee matching programs on employment outcomes. Our findings indicate that, in the case of PaYS, providing individuals with a promise of an interview upon separation from the military was not sufficient to increase earnings.

PaYS might not be effective in that regard, but the idea of linking employers and veterans seems like a good one. DoD and VA should work together to find out what the needs of veterans are when they enter the civilian workforce and develop interventions to address those needs.

We suggest more analysis of the effectiveness of veterans' transition assistance programs, including those offered by the U.S. Department of Labor. A better understanding of these programs is important especially in light of the lack of evidence of beneficial effects of PaYS and the finding that veterans who exit in times of high unemployment have lower earnings.

Summary of Policy Suggestions

The results of this analysis suggest several possibilities for policymakers to consider. These include the following:

- Develop credentialing programs that veterans can use to signal to employers that they possess certain skills.
- Conduct more research on the effect of recent changes to veterans' educational benefits and help veterans select and apply for colleges.
- Examine the implementation of the PaYS program to determine whether and when it becomes ineffective, followed by revamping the Army PaYS program or conducting more research on the effectiveness of veterans' transition assistance programs.
- Develop more programs to aid the transition from the military.
- Examine how business-cycle fluctuations affect the demand for transition assistance services.

Appendix. Supplemental Tables

Table A.1
Summary Statistics, by Years of Service: Zero to Five Years of Service and Nonenlistees

		Years of Service					
Characteristic	**Nonenlistees**	**0**	**1**	**2**	**3**	**4**	**5**
Dropout or GED	0.015	0.035	0.035	0.022	0.016	0.013	0.002
In high school	0.475	0.335	0.337	0.371	0.336	0.356	0.602
High school graduate	0.509	0.630	0.628	0.607	0.648	0.631	0.396
Male	0.793	0.863	0.806	0.847	0.858	0.883	0.895
AFQT category I	0.036	0.038	0.026	0.020	0.025	0.040	0.038
AFQT category II	0.374	0.362	0.361	0.367	0.339	0.413	0.439
AFQT category IIIA	0.261	0.259	0.284	0.270	0.259	0.259	0.276
AFQT category IIIB	0.330	0.340	0.329	0.343	0.377	0.288	0.247
White	0.779	0.774	0.830	0.809	0.768	0.806	0.837
Black	0.153	0.168	0.143	0.172	0.176	0.127	0.101
Hispanic	0.058	0.048	0.024	0.019	0.051	0.057	0.052
Other race or ethnicity	0.010	0.011	0.003	0.001	0.005	0.010	0.010
Age	18.793	19.085	18.957	18.834	18.934	18.898	18.220
Army	0.345	0.354	0.387	0.371	0.433	0.284	0.240
Navy	0.270	0.354	0.293	0.359	0.358	0.247	0.231
Air Force	0.116	0.108	0.162	0.136	0.111	0.192	0.170
Marine Corps	0.269	0.184	0.158	0.134	0.099	0.276	0.359
Number of observations	24,221	21,164	11,135	8,443	12,110	19,452	12,212
Percentage of observations in microdata in analysis	80	84	80	76	79	82	79

NOTE: GED® is a registered trademark of the American Council on Education.

Table A.2
Summary Statistics, by Years of Service: Six to 12 Years of Service and Those Who Did Not Separate

	Years of Service							
Characteristic	**6**	**7**	**8**	**9**	**10**	**11**	**12**	**Did Not Separate**
Dropout or GED	0.001	0.000	0.000	0.000	0.000	0.000	0.000	0.004
In high school	0.512	0.496	0.505	0.518	0.567	0.621	0.780	0.401
High school graduate	0.487	0.504	0.495	0.482	0.433	0.379	0.220	0.595
Male	0.871	0.932	0.934	0.953	0.932	0.947	0.961	0.893
AFQT category I	0.058	0.050	0.010	0.006	0.034	0.000	0.000	0.033
AFQT category II	0.526	0.552	0.590	0.575	0.560	0.655	0.622	0.405
AFQT category IIIA	0.240	0.212	0.209	0.250	0.221	0.227	0.276	0.264
AFQT category IIIB	0.176	0.186	0.191	0.169	0.185	0.118	0.102	0.299
White	0.879	0.876	0.899	0.941	0.917	0.919	0.961	0.697
Black	0.102	0.119	0.091	0.059	0.083	0.081	0.039	0.251
Hispanic	0.016	0.005	0.010	0.000	0.000	0.000	0.000	0.041
Other	0.003	0.000	0.000	0.000	0.000	0.000	0.000	0.011
Age	18.459	18.553	18.455	18.337	18.246	17.975	17.858	18.926
Army	0.383	0.531	0.379	0.370	0.304	0.466	0.252	0.279
Navy	0.318	0.280	0.165	0.245	0.389	0.193	0.386	0.290
Air Force	0.131	0.111	0.299	0.210	0.250	0.264	0.181	0.331
Marine Corps	0.168	0.078	0.156	0.175	0.056	0.078	0.181	0.100
Number of observations	3,723	2,161	1,029	816	647	322	127	10.749
Percentage of observations in microdata in analysis	65	56	41	41	36	28	16	74

Table A.3
Summary Statistics, by Occupational Specialty Category: Nonenlistees, Combat Arms, Electronic Repair, Communications or Intelligence, and Health Care

Characteristic	Nonenlistees	Combat Arms	Electronic Repair	Communications or Intelligence	Health Care
Dropout or GED	0.034	0.027	0.009	0.007	0.000
In high school	0.482	0.435	0.423	0.357	0.279
High school graduate	0.484	0.538	0.568	0.636	0.721
Male	0.848	0.893	0.925	0.882	0.532
AFQT category I	0.037	0.035	0.062	0.074	0.015
AFQT category II	0.359	0.355	0.506	0.565	0.564
AFQT category IIIA	0.260	0.250	0.184	0.214	0.336
AFQT category IIIB	0.344	0.361	0.248	0.147	0.086
White	0.774	0.764	0.836	0.873	0.836
Black	0.143	0.156	0.136	0.108	0.157
Hispanic	0.067	0.065	0.026	0.018	0.005
Other	0.016	0.015	0.002	0.001	0.002
Age	18.8	18.7	18.7	18.8	19.2
Army	0.326	0.293	0.209	0.503	0.370
Navy	0.312	0.502	0.519	0.121	0.179
Air Force	0.100	0.052	0.167	0.186	0.451
Marine Corps	0.263	0.154	0.104	0.190	0.000
Number of observations	23,440	34,737	7,039	4,901	3,271
Percentage of observations in microdata in analysis	77	86	75	70	58

Table A.4
Summary Statistics, by Occupational Specialty Category: Other Technical, Functional or Administrative, Mechanical Repairs, Craftsworkers, and Service or Supply

Characteristic	Other Technology or Allied	Functional or Administrative	Mechanical Repairs	Craftsworkers	Service or Supply
Dropout or GED	0.000	0.000	0.008	0.003	0.001
In high school	0.382	0.368	0.402	0.405	0.405
High school graduate	0.618	0.632	0.590	0.592	0.594
Male	0.903	0.690	0.956	0.989	0.838
AFQT category I	0.029	0.026	0.036	0.000	0.005
AFQT category II	0.652	0.418	0.435	0.399	0.286
AFQT category IIIA	0.215	0.313	0.289	0.343	0.293
AFQT category IIIB	0.104	0.243	0.241	0.258	0.416
White	0.909	0.657	0.868	0.959	0.741
Black	0.091	0.301	0.100	0.038	0.217
Hispanic	0.000	0.038	0.029	0.003	0.039
Other	0.000	0.004	0.003	0.000	0.003
Age	18.7	18.9	18.8	18.7	18.8
Army	0.308	0.364	0.309	0.118	0.390
Navy	0.017	0.071	0.113	0.291	0.032
Air Force	0.451	0.311	0.345	0.361	0.255
Marine Corps	0.224	0.254	0.233	0.230	0.322
Number of observations	968	7,188	11,212	1,594	7,859
Percentage of observations in microdata in analysis	47	68	78	55	74

Table A.5
Effects of Military Enlistment on Annual Earnings (in Thousands of 2005 Dollars), by Years Since Application and Years of Service Prior to Separation, 1994 Application Cohort: Zero to Six Years of Service

	Years of Service						
Years Since Application	0	1	2	3	4	5	6
0	–0.0	0.0	0.6*	0.9*	0.7*	0.1	0.6*
	(0.1)	(0.1)	(0.1)	(0.1)	(0.1)	(0.0)	(0.1)
1	0.9*	–0.4	4.2*	4.6*	4.1*	1.7*	3.2*
	(0.2)	(0.2)	(0.2)	(0.3)	(0.2)	(0.2)	(0.2)
2	1.8*	–2.0*	1.5*	7.0*	6.5*	6.0*	7.0*
	(0.1)	(0.1)	(0.1)	(0.1)	(0.1)	(0.1)	(0.1)
3	1.3*	–2.1*	–2.8*	2.1*	6.7*	6.6*	7.9*

Years Since Application	Years of Service						
	0	1	2	3	4	5	6
	(0.1)	(0.1)	(0.1)	(0.1)	(0.1)	(0.1)	(0.1)
4	0.6*	–2.3*	–2.4*	–2.3*	2.6*	7.5*	9.2*
	(0.1)	(0.2)	(0.2)	(0.1)	(0.1)	(0.1)	(0.2)
5	–0.3	–2.8*	–2.9*	–1.3*	–1.7*	2.9*	9.5*
	(0.1)	(0.2)	(0.2)	(0.2)	(0.2)	(0.1)	(0.2)
6	–0.9*	–3.5*	–3.1*	–1.0*	–0.1	–0.6*	5.5*
	(0.2)	(0.2)	(0.2)	(0.2)	(0.2)	(0.2)	(0.3)
7	–0.7*	–4.0*	–3.0*	–0.9*	0.7*	0.8*	0.2
	(0.2)	(0.2)	(0.7)	(0.2)	(0.2)	(0.2)	(0.4)
8	0.7*	–4.5*	–4.0*	–0.8*	1.3*	1.4*	1.7*
	(0.2)	(0.2)	(0.2)	(0.2)	(0.2)	(0.2)	(0.4)
9	2.2*	–4.7*	–4.2*	–0.5*	1.8*	2.2*	2.6*
	(0.2)	(0.2)	(0.3)	(0.3)	(0.2)	(0.3)	(0.4)
10	2.7*	–4.9*	–4.2*	–0.3	2.5*	3.2*	3.5*
	(0.3)	(0.3)	(0.3)	(0.3)	(0.3)	(0.3)	(0.5)
11	3.3*	–5.1*	–4.1*	–0.1	3.0*	4.1*	4.7*
	(0.3)	(0.3)	(0.4)	(0.3)	(0.3)	(0.3)	(0.5)
12	3.4*	–5.4*	–4.4*	–0.1	3.3*	4.6*	5.6*
	(0.4)	(0.3)	(0.4)	(0.4)	(0.3)	(0.4)	(0.5)
13	3.7*	–5.5*	–4.5*	0.3	3.6*	5.3*	6.9*
	(0.5)	(0.3)	(0.5)	(0.4)	(0.4)	(0.4)	(0.6)
14	4.0*	–5.6*	–4.4*	0.9*	4.3*	6.2*	7.9*
	(0.4)	(0.3)	(0.4)	(0.4)	(0.3)	(0.5)	(0.6)
15	6.2*	–5.9*	–4.6*	1.1*	5.3*	8.1*	9.2*
	(0.4)	(0.4)	(0.4)	(0.4)	(0.4)	(0.4)	(0.7)
16	7.2*	–5.8*	–4.5*	1.7*	6.1*	8.9*	10.3*
	(0.3)	(0.3)	(0.4)	(0.4)	(0.4)	(0.4)	(0.6)

NOTE: Estimates are weighted average of cell-level treatment effect estimates, weighted by the share of veterans. Standard errors in parentheses are equal to the square root of the variance of the weighted average of cell-level treatment effect estimates. * = statistically significant at the 5-percent level.

Table A.6

Effects of Military Enlistment on Annual Earnings (in Thousands of 2005 Dollars), by Years Since Application and Years of Service Prior to Separation, 1994 Application Cohort: Seven to 12 Years of Service and Those Who Did Not Separate

	Years of Service						
Years Since Application	**7**	**8**	**9**	**10**	**11**	**12**	**Not Separated**
0	0.8*	0.4*	0.3*	0.4*	0.2	0.4	0.9*
	(0.1)	(0.1)	(0.2)	(0.2)	(0.2)	(0.3)	(0.1)
1	3.5*	3.4*	2.9*	1.8*	3.2*	2.6*	3.9*
	(0.1)	(0.3)	(0.3)	(0.9)	(0.3)	(0.5)	(0.2)
2	7.1*	6.8*	6.8*	6.7*	6.7*	5.8*	7.1*
	(0.2)	(0.2)	(0.2)	(0.2)	(0.3)	(0.5)	(0.1)
3	8.1*	7.3*	8.6*	8.4*	8.4*	7.4*	8.3*
	(0.2)	(0.2)	(0.3)	(0.3)	(0.4)	(0.7)	(0.1)
4	9.5*	9.1*	9.9*	10.6*	9.8*	10.2*	10.4*
	(0.2)	(0.3)	(0.3)	(0.4)	(0.5)	(0.9)	(0.2)
5	9.8*	9.5*	11.4*	12.1*	12.4*	11.2*	11.5*
	(0.3)	(0.3)	(0.3)	(0.4)	(0.6)	(0.8)	(0.2)
6	9.8*	8.8*	11.1*	13.3*	11.5*	14.7*	11.7*
	(0.3)	(0.3)	(0.4)	(0.5)	(0.6)	(1.2)	(0.2)
7	5.3*	9.8*	12.4*	14.1*	13.0*	16.8*	14.0*
	(0.4)	(0.4)	(0.4)	(0.5)	(0.6)	(1.3)	(0.2)
8	–0.8	8.1*	16.0*	17.9*	18.4*	20.0*	19.5*
	(0.5)	(0.5)	(0.4)	(0.5)	(0.7)	(1.1)	(0.2)
9	1.6*	–0.8	11.6*	21.5*	22.9*	22.9*	25.1*
	(0.5)	(0.7)	(0.7)	(0.6)	(0.7)	(1.1)	(0.2)
10	2.9*	2.1*	0.9	16.7*	24.5*	24.6*	27.5*
	(0.6)	(0.9)	(0.9)	(0.9)	(0.8)	(1.2)	(0.3)
11	4.4*	2.9*	3.4*	0.9	18.6*	26.2*	30.2*
	(0.7)	(1.0)	(0.9)	(1.0)	(1.4)	(1.5)	(0.3)
12	5.1*	4.1*	4.6*	3.4*	1.6	17.7*	31.1*
	(0.7)	(0.9)	(1.0)	(1.1)	(1.5)	(2.5)	(0.4)
13	5.9*	4.8*	5.7*	5.4*	4.7*	1.4	32.7*
	(0.7)	(1.0)	(1.1)	(1.2)	(1.6)	(2.6)	(0.4)
14	7.9*	6.2*	7.3*	7.5*	7.1*	3.2	33.0*
	(0.7)	(1.0)	(1.1)	(1.3)	(1.7)	(2.6)	(0.4)
15	10.1*	8.7*	10.4*	11.0*	8.9*	6.0*	42.6*
	(0.9)	(1.1)	(1.2)	(1.4)	(1.8)	(2.7)	(0.4)

Years Since Application	Years of Service						
	7	8	9	10	11	12	Not Separated
16	10.3*	9.6*	11.6*	11.2*	10.4*	10.5*	44.3*
	(0.8)	(1.1)	(1.3)	(1.4)	(1.9)	(3.0)	(0.4)

NOTE: Estimates are weighted averages of cell-level treatment effect estimates, weighted by the share of veterans. Standard errors in parentheses are equal to the square root of the variance of the weighted average of cell-level treatment effect estimates. * = statistically significant at the 5-percent level.

Table A.7

Effects of Military Enlistment on Annual Earnings as a Percentage of Mean Earnings, by Years Since Application and Years of Service Prior to Separation, 1994 Application Cohort: Zero to Six Years of Service

Years Since Application	Years of Service						
	0	1	2	3	4	5	6
0	–0.3	0.7	8.6*	11.9*	9.9*	0.8	8.4*
	(0.7)	(0.8)	(0.9)	(0.8)	(0.7)	(0.8)	(1.2)
1	8.5*	–4.0	36.6*	38.2*	33.4*	16.5*	29.9*
	(2.0)	(2.2)	(2.1)	(2.2)	(1.5)	(1.7)	(1.7)
2	13.1*	–16.1*	11.0*	45.4*	40.5*	40.4*	49.4*
	(0.7)	(0.8)	(0.8)	(0.6)	(0.5)	(0.5)	(0.9)
3	8.0*	–14.4*	–19.3*	12.9*	35.5*	36.9*	45.9*
	(0.7)	(0.9)	(0.9)	(0.7)	(0.6)	(0.6)	(0.8)
4	3.2*	–13.1*	–13.7*	–13.3*	13.0*	36.0*	45.7*
	(0.7)	(1.0)	(0.9)	(0.8)	(0.6)	(0.6)	(0.8)
5	–1.4	–14.4*	–14.5*	–6.4*	–8.3*	13.0*	41.5*
	(0.7)	(0.9)	(1.0)	(0.9)	(0.8)	(0.7)	(0.8)
6	–4.0*	–15.9*	–14.1*	–4.3*	–0.3	–2.7*	22.3*
	(0.7)	(0.9)	(1.1)	(0.9)	(0.8)	(0.8)	(1.1)
7	–2.8*	–17.5*	–12.6*	–3.8*	2.6*	3.2*	1.0
	(0.7)	(0.9)	(2.9)	(0.9)	(0.8)	(0.9)	(1.4)
8	2.7*	–19.3*	–16.9*	–3.4*	4.9*	5.5*	6.6*
	(0.8)	(1.0)	(1.0)	(1.0)	(0.8)	(0.9)	(1.5)
9	8.0*	–19.4*	–17.2*	–2.1*	6.7*	8.0*	9.4*
	(0.8)	(1.0)	(1.1)	(1.0)	(0.9)	(0.9)	(1.5)
10	9.5*	–19.1*	–16.1*	–1.3	8.6*	10.8*	12.0*
	(1.0)	(1.1)	(1.2)	(1.1)	(0.9)	(1.0)	(1.7)
11	11.0*	–19.5*	–15.3*	–0.2	9.8*	13.3*	15.6*
	(1.1)	(1.2)	(1.4)	(1.2)	(1.0)	(1.1)	(1.8)
12	10.8*	–19.6*	–15.4*	–0.2	10.1*	14.2*	17.4*
	(1.2)	(1.1)	(1.5)	(1.3)	(1.1)	(1.1)	(1.7)

Years Since Application	Years of Service						
	0	1	2	3	4	5	6
13	11.3*	–19.2*	–15.2*	0.9	10.6*	15.7*	20.5*
	(1.4)	(1.2)	(1.8)	(1.4)	(1.2)	(1.2)	(1.7)
14	12.2*	–19.4*	–15.0*	2.8*	12.5*	18.0*	23.1*
	(1.1)	(1.1)	(1.4)	(1.2)	(1.0)	(1.3)	(1.8)
15	18.8*	–21.3*	–16.5*	3.7*	15.9*	23.9*	27.7*
	(1.1)	(1.3)	(1.3)	(1.3)	(1.1)	(1.3)	(2.0)
16	22.6*	–22.1*	–16.7*	5.7*	18.7*	27.2*	32.0*
	(1.1)	(1.3)	(1.5)	(1.3)	(1.1)	(1.2)	(2.0)

NOTE: Estimates are weighted averages of cell-level treatment effect estimates, weighted by the share of veterans. Standard errors in parentheses are equal to the square root of the variance of the weighted average of cell-level treatment effect estimates. * = statistically significant at the 5-percent level.

Table A.8

Effects of Military Enlistment on Annual Earnings as a Percentage of Mean Earnings, by Years Since Application and Years of Service Prior to Separation, 1994 Application Cohort: Seven to 12 Years of Service and Those Who Did Not Separate

Years Since Application	Years of Service						
	7	8	9	10	11	12	Not Separated
0	10.9*	6.0*	5.2*	7.0*	4.3	7.3	12.7*
	(1.6)	(2.4)	(2.4)	(2.5)	(4.0)	(6.9)	(0.9)
1	33.2*	32.2*	27.2*	17.4*	34.8*	30.5*	33.2*
	(1.2)	(2.6)	(3.0)	(8.9)	(3.5)	(5.7)	(1.3)
2	49.4*	49.7*	49.3*	49.8*	52.0*	46.8*	45.2*
	(1.1)	(1.6)	(1.6)	(1.8)	(2.6)	(3.9)	(0.7)
3	46.4*	43.3*	51.7*	50.4*	52.8*	48.0*	44.0*
	(1.0)	(1.4)	(1.7)	(1.8)	(2.6)	(4.3)	(0.7)
4	46.1*	45.8*	49.8*	53.1*	51.6*	54.7*	46.6*
	(1.1)	(1.4)	(1.6)	(1.9)	(2.5)	(4.8)	(0.7)
5	41.7*	41.8*	50.4*	53.1*	56.6*	51.5*	45.3*
	(1.1)	(1.4)	(1.5)	(1.9)	(2.6)	(3.7)	(0.7)
6	37.6*	35.0*	44.0*	51.7*	46.5*	59.4*	41.6*
	(1.0)	(1.3)	(1.4)	(1.8)	(2.4)	(4.7)	(0.7)
7	19.7*	36.6*	45.9*	51.5*	48.6*	63.5*	46.4*
	(1.4)	(1.4)	(1.5)	(1.7)	(2.4)	(5.0)	(0.7)
8	–2.8	29.3*	56.6*	62.8*	66.3*	72.3*	59.6*
	(1.8)	(1.9)	(1.5)	(1.8)	(2.6)	(4.0)	(0.7)
9	5.7*	–3.0	40.0*	71.9*	77.9*	79.5*	70.5*
	(1.9)	(2.6)	(2.2)	(1.9)	(2.5)	(3.8)	(0.7)

Years Since Application	Years of Service						
	7	8	9	10	11	12	Not Separated
10	9.7*	7.0*	3.2	54.1*	79.1*	81.6*	72.9*
	(2.1)	(2.9)	(3.0)	(3.0)	(2.6)	(4.0)	(0.7)
11	13.8*	9.5*	11.0*	2.9	58.2*	82.3*	76.0*
	(2.2)	(3.1)	(3.0)	(3.4)	(4.4)	(4.9)	(0.8)
12	15.4*	12.5*	14.2*	10.4*	4.8	53.5*	74.8*
	(2.1)	(2.8)	(3.1)	(3.3)	(4.6)	(7.5)	(0.8)
13	17.1*	14.3*	16.9*	15.7*	13.7*	4.3	75.5*
	(2.1)	(2.8)	(3.1)	(3.4)	(4.8)	(7.6)	(1.0)
14	22.6*	18.0*	21.3*	21.6*	20.3*	9.1	75.7*
	(2.1)	(2.8)	(3.3)	(3.6)	(4.9)	(7.6)	(0.8)
15	29.5*	26.1*	31.1*	32.6*	26.5*	17.9*	93.6*
	(2.7)	(3.2)	(3.6)	(4.1)	(5.5)	(8.2)	(0.8)
16	31.2*	29.9*	36.0*	34.2*	31.7*	32.4*	98.7*
	(2.5)	(3.5)	(3.9)	(4.3)	(5.8)	(9.3)	(0.8)

NOTE: Estimates are weighted averages of cell-level treatment effect estimates, weighted by the share of veterans. Standard errors in parentheses are equal to the square root of the variance of the weighted average of cell-level treatment effect estimates. * = statistically significant at the 5-percent level.

Table A.9

Effects of Military Enlistment on Annual Earnings (in 2005 Dollars), by Military Occupational Specialty and Years of Service Prior to Separation, 1994 Application Cohort: Combat Arms, Electronic Repair, Communications or Intelligence, and Health Care

Years Since Application	Combat Arms	Electronic Repair	Communications or Intelligence	Health Care
0	690.7*	672.8*	487.0*	624.3*
	(46.0)	(71.5)	(81.6)	(106.5)
1	3,368.8*	2,729.3*	3,576.7*	4,734.0*
	(311.0)	(515.6)	(106.9)	(132.7)
2	5,330.3*	5,143.9*	5,995.4*	7,369.7*
	(79.4)	(118.9)	(130.5)	(161.1)
3	4,430.7*	4,711.9*	5,497.1*	7,324.9*
	(100.4)	(148.9)	(165.0)	(195.4)
4	3,319.9*	4,415.1*	4,632.5*	6,664.8*
	(119.9)	(186.7)	(203.7)	(235.3)
5	2,009.5*	3,770.7*	3,408.0*	4,506.0*
	(138.7)	(224.8)	(272.6)	(276.9)
6	1,495.4*	4,197.0*	3,603.9*	2,847.0*
	(160.5)	(268.3)	(318.5)	(324.0)

Years Since Application	Combat Arms	Electronic Repair	Communications or Intelligence	Health Care
7	1,879.1*	4,602.3*	4,264.5*	2,624.7*
	(233.8)	(296.0)	(361.0)	(360.6)
8	2,487.0*	5,198.8*	4,599.7*	3,325.1*
	(189.7)	(321.4)	(406.0)	(404.2)
9	3,400.3*	5,535.1*	5,706.6*	4,498.8*
	(208.9)	(345.5)	(468.4)	(464.9)
10	3,781.6*	6,460.4*	6,599.0*	5,315.8*
	(254.9)	(378.3)	(625.7)	(581.3)
11	4,424.5*	6,599.3*	7,159.1*	5,531.8*
	(270.0)	(407.6)	(745.1)	(677.6)
12	4,595.0*	6,934.4*	7,229.4*	5,192.7*
	(308.1)	(431.2)	(835.7)	(703.7)
13	4,966.7*	7,414.2*	7,260.9*	5,799.0*
	(362.0)	(452.3)	(1037.2)	(848.4)
14	5,415.0*	7,810.9*	8,146.1*	7,188.6*
	(309.8)	(473.2)	(832.7)	(695.6)
15	7,182.4*	9,988.4*	11,480.3*	9,992.0*
	(310.2)	(483.9)	(945.9)	(817.7)
16	7,797.6*	11,005.7*	12,498.2*	11,318.3*
	(308.8)	(508.8)	(674.7)	(688.7)

NOTE: Estimates are weighted averages of cell-level treatment effect estimates, weighted by the share of veterans. Standard errors in parentheses are equal to the square root of the variance of the weighted average of cell-level treatment effect estimates. * = statistically significant at the 5-percent level.

Table A.10

Effects of Military Enlistment on Annual Earnings (in 2005 Dollars), by Military Occupational Specialty and Years of Service Prior to Separation, 1994 Application Cohort: Other Technical or Allied, Functional or Administrative, Mechanical Repairs, Craftsworkers, and Service or Supply

Years Since Application	Other Technical or Allied	Functional or Administrative	Mechanical Repairs	Craftsworkers	Service or Supply
0	188.1	832.4*	602.1*	387.0*	762.5*
	(151.9)	(68.1)	(70.3)	(128.3)	(69.5)
1	3,606.4*	4,858.6*	3,311.2*	2,486.3*	4,048.4*
	(203.9)	(93.8)	(103.2)	(332.0)	(98.0)
2	6,082.5*	7,151.5*	5,605.7*	4,722.6*	6,181.4*
	(244.8)	(116.0)	(119.0)	(195.2)	(111.6)
3	5,741.2*	6,637.2*	4,963.9*	4,446.7*	5,412.7*

Years Since Application	Other Technical or Allied	Functional or Administrative	Mechanical Repairs	Craftsworkers	Service or Supply
	(302.0)	(148.6)	(151.0)	(246.7)	(141.6)
4	4,581.3*	6,307.9*	4,278.3*	4,196.3*	4,188.0*
	(376.9)	(220.8)	(171.7)	(307.5)	(164.8)
5	1,901.3*	4,589.5*	3,071.9*	2,738.6*	2,077.8*
	(465.2)	(212.3)	(209.9)	(377.2)	(194.4)
6	837.1	4,161.1*	2,811.7*	2,110.8*	1,526.2*
	(556.8)	(252.7)	(235.5)	(436.2)	(217.9)
7	882.1	4,711.0*	2,837.2*	2,315.9*	2,130.3*
	(621.4)	(281.5)	(257.9)	(483.8)	(242.1)
8	2,836.4*	5,740.4*	4,159.7*	3,136.0*	3,600.4*
	(698.4)	(302.8)	(279.3)	(531.1)	(267.9)
9	3,803.9*	7,109.2*	5,212.2*	4,118.7*	4,483.1*
	(762.9)	(332.2)	(305.7)	(585.3)	(291.5)
10	4,617.1*	7,917.4*	5,808.9*	3,409.4*	5,018.5*
	(823.5)	(357.6)	(338.0)	(619.6)	(320.0)
11	4,618.1*	8,446.3*	6,399.9*	4,608.0*	5,485.0*
	(884.2)	(393.6)	(383.0)	(694.4)	(351.2)
12	4,833.7*	8,451.6*	6,472.2*	5,378.4*	5,501.7*
	(947.4)	(409.6)	(418.7)	(751.8)	(368.0)
13	6,538.3*	8,668.4*	6,885.9*	6,024.2*	6,131.5*
	(1,018.1)	(451.4)	(444.2)	(779.2)	(382.7)
14	6,524.6*	9,531.1*	7,481.2*	6,156.6*	6,535.1*
	(1,041.7)	(465.4)	(443.1)	(828.0)	(395.8)
15	9,499.8*	12,387.0*	9,675.4*	8,208.4*	8,726.9*
	(1,137.4)	(495.3)	(446.4)	(877.6)	(419.1)
16	10,562.0*	12,896.2*	10,322.1*	8,901.6*	9,422.0*
	(1,204.8)	(502.9)	(458.3)	(917.2)	(440.0)

NOTE: Estimates are weighted averages of cell-level treatment effect estimates, weighted by the share of veterans. Standard errors in parentheses are equal to the square root of the variance of the weighted average of cell-level treatment effect estimates. * = statistically significant at the 5-percent level.

Table A.11

Effects of Military Enlistment as a Percentage of Mean Earnings, by Military Occupational Specialty and Years of Service Prior to Separation, 1994 Application Cohort: Combat Arms, Electronic Repair, Communications or Intelligence, and Health Care

Years Since Application	Combat Arms	Electronic Repair	Communications or Intelligence	Health Care
0	9.6*	9.4*	6.9*	8.1*
	(0.6)	(1.0)	(1.2)	(1.4)

Years Since Application	Combat Arms	Electronic Repair	Communications or Intelligence	Health Care
1	27.3*	23.8*	32.1*	40.9*
	(2.5)	(4.5)	(1.0)	(1.1)
2	33.2*	35.0*	40.8*	50.1*
	(0.5)	(0.8)	(0.9)	(1.1)
3	24.4*	27.2*	31.6*	42.5*
	(0.6)	(0.9)	(0.9)	(1.1)
4	16.3*	21.9*	23.1*	33.8*
	(0.6)	(0.9)	(1.0)	(1.2)
5	9.1*	16.6*	15.1*	20.8*
	(0.6)	(1.0)	(1.2)	(1.3)
6	6.1*	16.4*	14.3*	12.0*
	(0.7)	(1.0)	(1.3)	(1.4)
7	7.2*	16.9*	15.9*	10.4*
	(0.9)	(1.1)	(1.3)	(1.4)
8	9.2*	18.5*	16.5*	12.7*
	(0.7)	(1.1)	(1.5)	(1.5)
9	12.0*	19.0*	19.6*	16.5*
	(0.7)	(1.2)	(1.6)	(1.7)
10	12.5*	21.0*	21.4*	18.5*
	(0.8)	(1.2)	(2.0)	(2.0)
11	14.0*	20.6*	22.2*	18.4*
	(0.9)	(1.3)	(2.3)	(2.3)
12	13.9*	20.7*	21.4*	16.6*
	(0.9)	(1.3)	(2.5)	(2.2)
13	14.4*	21.2*	20.6*	17.7*
	(1.0)	(1.3)	(2.9)	(2.6)
14	15.5*	22.0*	22.9*	21.7*
	(0.9)	(1.3)	(2.3)	(2.1)
15	20.7*	28.6*	32.9*	30.4*
	(0.9)	(1.4)	(2.7)	(2.5)
16	23.1*	32.4*	36.8*	35.4*
	(0.9)	(1.5)	(2.0)	(2.2)

NOTE: Estimates are weighted averages of cell-level treatment effect estimates, weighted by the share of veterans. Standard errors in parentheses are equal to the square root of the variance of the weighted average of cell-level treatment effect estimates. * = statistically significant at the 5-percent level.

Table A.12

Effects of Military Enlistment as a Percentage of Mean Earnings, by Military Occupational Specialty and Years of Service Prior to Separation, 1994 Application Cohort: Other Technical or Allied, Functional or Administrative, Mechanical Repairs, Craftsworkers, and Service or Supply

Years Since Application	Other Technical or Allied	Functional or Administrative	Mechanical Repairs	Craftsworkers	Service or Supply
0	3.0	11.9*	8.2*	5.7*	10.7*
	(2.4)	(1.0)	(1.0)	(1.9)	(1.0)
1	35.1*	43.2*	27.8*	22.7*	35.1*
	(2.0)	(0.8)	(0.9)	(3.0)	(0.9)
2	43.8*	48.7*	36.0*	33.4*	41.6*
	(1.8)	(0.8)	(0.8)	(1.4)	(0.8)
3	34.2*	38.5*	27.2*	26.0*	31.1*
	(1.8)	(0.9)	(0.8)	(1.4)	(0.8)
4	23.3*	31.9*	20.6*	21.0*	21.3*
	(1.9)	(1.1)	(0.8)	(1.5)	(0.8)
5	8.6*	21.1*	13.3*	12.1*	9.7*
	(2.1)	(1.0)	(0.9)	(1.7)	(0.9)
6	3.4	17.2*	11.0*	8.4*	6.4*
	(2.2)	(1.0)	(0.9)	(1.7)	(0.9)
7	3.3	18.3*	10.5*	8.6*	8.5*
	(2.3)	(1.1)	(1.0)	(1.8)	(1.0)
8	10.2*	21.5*	14.7*	11.3*	13.7*
	(2.5)	(1.1)	(1.0)	(1.9)	(1.0)
9	13.2*	25.5*	17.6*	14.2*	16.4*
	(2.6)	(1.2)	(1.0)	(2.0)	(1.1)
10	15.2*	27.0*	18.6*	11.2*	17.5*
	(2.7)	(1.2)	(1.1)	(2.0)	(1.1)
11	14.6*	27.6*	19.6*	14.6*	18.3*
	(2.8)	(1.3)	(1.2)	(2.2)	(1.2)
12	14.5*	26.4*	18.9*	16.2*	17.6*
	(2.8)	(1.3)	(1.2)	(2.3)	(1.2)
13	18.8*	26.1*	19.3*	17.5*	18.8*
	(2.9)	(1.4)	(1.2)	(2.3)	(1.2)
14	18.7*	28.3*	20.8*	17.7*	19.9*
	(3.0)	(1.4)	(1.2)	(2.4)	(1.2)
15	27.7*	37.0*	27.3*	24.4*	27.0*
	(3.3)	(1.5)	(1.3)	(2.6)	(1.3)

Years Since Application	Other Technical or Allied	Functional or Administrative	Mechanical Repairs	Craftsworkers	Service or Supply
16	31.6*	39.8*	30.0*	27.5*	30.1*
	(3.6)	(1.6)	(1.3)	(2.8)	(1.4)

NOTE: Estimates are weighted averages of cell-level treatment effect estimates, weighted by the share of veterans. Standard errors in parentheses are equal to the square root of the variance of the weighted average of cell-level treatment effect estimates. * = statistically significant at the 5-percent level.

Table A.13
Effects of Military Enlistment on College Enrollment (1998–2000 Cohorts) and Degree Attainment (1991–1994 Cohorts), by Years Since Application and Years of Service Prior to Separation

	Years of Service					
	Enrollment			Degree Attainment		
Years Since Application	0–4	5–7	8+	0–4	5–7	8+
0	–0.0334***	–0.0349***	–0.0399***			
	(0.00296)	(0.00299)	(0.00282)			
1	–0.089***	–0.102***	–0.101***			
	(0.00357)	(0.00302)	(0.00296)			
2	–0.0928***	–0.131***	–0.127***	–0.000766***	–0.000703***	–0.000531***
	(0.00416)	(0.00364)	(0.00363)	(0.000189)	(0.000164)	(0.000136)
3	–0.0879***	–0.141***	–0.139***	–0.00488***	–0.00318***	–0.00328***
	(0.00462)	(0.00432)	(0.0043)	(0.000569)	(0.000361)	(0.000343)
4	–0.0641***	–0.148***	–0.144***	–0.0101***	–0.00848***	–0.00802***
	(0.00505)	(0.005)	(0.00501)	(0.000808)	(0.000561)	(0.00052)
5	–0.0341***	–0.112***	–0.141***	–0.0201***	–0.0166***	–0.0161***
	(0.00545)	(0.006)	(0.00577)	(0.00117)	(0.000797)	(0.000756)
6	–0.0212***	–0.0502***	–0.134***	–0.0279***	–0.0271***	–0.027***
	(0.0057)	(0.0068)	(0.00631)	(0.00157)	(0.00112)	(0.00107)
7	–0.016***	0.00887	–0.123***	–0.03***	–0.0346***	–0.0367***
	(0.00583)	(0.00716)	(0.00667)	(0.00203)	(0.00168)	(0.00161)
8	–0.0115*	0.0141*	–0.106***	–0.0272***	–0.0356***	–0.0431***
	(0.00592)	(0.00733)	(0.00696)	(0.00247)	(0.00238)	(0.00227)
9	–0.00476	0.0354***	–0.0887***	–0.0244***	–0.0277***	–0.0457***
	(0.00598)	(0.00744)	(0.00719)	(0.00282)	(0.00312)	(0.00287)
10	–0.00329	0.0495***	–0.0644***	–0.0202***	–0.0169***	–0.0459***
	(0.00746)	(0.00926)	(0.00907)	(0.00311)	(0.00377)	(0.00339)
11	0.00561	0.0634***	–0.052***	–0.0173***	–0.0077*	–0.0435***
	(0.0104)	(0.013)	(0.0128)	(0.00332)	(0.00424)	(0.00386)
12				–0.0143***	–0.000256	–0.0373***
				(0.00349)	(0.0046)	(0.00433)

	Years of Service					
	Enrollment			Degree Attainment		
Years Since Application	0–4	5–7	8+	0–4	5–7	8+
13				–0.0134***	0.00636	–0.0266***
				(0.00365)	(0.00491)	(0.00483)
14				–0.0116***	0.0133**	–0.02***
				(0.00377)	(0.00517)	(0.00516)
15				–0.00973**	0.0197***	–0.0128**
				(0.00388)	(0.0054)	(0.00545)
16				–0.0104**	0.0202***	–0.00277
				(0.00449)	(0.00619)	(0.00647)
17				–0.00839	0.0213***	0.0135
				(0.00554)	(0.0075)	(0.00822)
18				–0.00556	0.0247**	0.0223*
				(0.00794)	(0.0106)	(0.0119)

NOTE: Estimates are weighted averages of cell-level treatment effect estimates, weighted by the share of veterans. Standard errors in parentheses are equal to the square root of the variance of the weighted average of cell-level treatment effect estimates. * = statistically significant at the 5-percent level. ** = statistically significant at the 1-percent level. *** = statistically significant at the 0.1-percent level.

Table A.14

Effects of Military Enlistment on College Enrollment, by Military Occupational Specialty and Years Since Military Application, 1998–2000 Application Cohorts: Combat Arms, Electronic Repair, Communications or Intelligence, and Health Care

Years Since Application	Combat Arms	Electronic Repair	Communications or Intelligence	Health Care
0	–0.0313***	–0.0368***	–0.0296***	–0.0136*
	(0.0027)	(0.00361)	(0.00368)	(0.00731)
1	–0.092***	–0.0923***	–0.0829***	–0.061***
	(0.00305)	(0.00341)	(0.00359)	(0.00711)
2	–0.12***	–0.124***	–0.109***	–0.0742***
	(0.00356)	(0.00418)	(0.00437)	(0.00892)
3	–0.13***	–0.136***	–0.117***	–0.0683***
	(0.00406)	(0.00536)	(0.00546)	(0.0111)
4	–0.119***	–0.137***	–0.111***	–0.0512***
	(0.00462)	(0.00678)	(0.0067)	(0.0134)
5	–0.091***	–0.123***	–0.0795***	–0.0194
	(0.00521)	(0.00839)	(0.0082)	(0.0155)
6	–0.0598***	–0.106***	–0.0506***	0.00412
	(0.0056)	(0.00949)	(0.00913)	(0.0167)
7	–0.0406***	–0.095***	–0.0402***	0.0343**
	(0.0058)	(0.0101)	(0.00952)	(0.0174)
8	–0.0263***	–0.0781***	–0.0299***	0.062***
	(0.00592)	(0.0106)	(0.00979)	(0.0178)
9	–0.0143**	–0.0685***	–0.0183*	0.0726***
	(0.00601)	(0.011)	(0.01)	(0.018)
10	–0.00193	–0.068***	–0.0114	0.0534**
	(0.00745)	(0.014)	(0.0126)	(0.0227)
11	–0.00218	–0.0463**	0.00613	0.081**
	(0.0104)	(0.0204)	(0.0181)	(0.0335)

NOTE: Estimates are weighted averages of cell-level treatment effect estimates, weighted by the share of veterans. Standard errors in parentheses are equal to the square root of the variance of the weighted average of cell-level treatment effect estimates. * = statistically significant at the 5-percent level. ** = statistically significant at the 1-percent level. *** = statistically significant at the 0.1-percent level.

Table A.15

Effects of Military Enlistment on College Enrollment, by Military Occupational Specialty and Years Since Military Application, 1998–2000 Application Cohorts: Other Technical or Allied, Functional or Administrative, Mechanical Repairs, Craftsworkers, and Service or Supply

Years Since Application	Other Technical or Allied	Functional or Administrative	Mechanical Repairs	Craftsworkers	Service or Supply
0	–0.0197***	–0.028***	–0.0411***	–0.0312***	–0.0259***
	(0.00751)	(0.00381)	(0.00274)	(0.00618)	(0.00404)
1	–0.0693***	–0.0705***	–0.0985***	–0.0849***	–0.071***
	(0.00696)	(0.00398)	(0.00293)	(0.00557)	(0.00411)
2	–0.0904***	–0.0827***	–0.125***	–0.113***	–0.0932***
	(0.00847)	(0.00503)	(0.00355)	(0.00675)	(0.00492)
3	–0.0893***	–0.0787***	–0.13***	–0.121***	–0.104***
	(0.011)	(0.00631)	(0.00432)	(0.00866)	(0.00585)
4	–0.0909***	–0.069***	–0.127***	–0.126***	–0.106***
	(0.013)	(0.00748)	(0.00512)	(0.0106)	(0.0069)
5	–0.0611***	–0.0433***	–0.109***	–0.114***	–0.0922***
	(0.0158)	(0.00867)	(0.00599)	(0.0129)	(0.0081)
6	–0.0324*	–0.0196**	–0.0909***	–0.0998***	–0.0735***
	(0.0175)	(0.0094)	(0.00654)	(0.0144)	(0.00894)
7	–0.00931	–0.00616	–0.0737***	–0.0848***	–0.0551***
	(0.0184)	(0.00975)	(0.00687)	(0.0154)	(0.00944)
8	0.00828	0.00351	–0.0633***	–0.0748***	–0.0376***
	(0.0189)	(0.00995)	(0.00707)	(0.016)	(0.00975)
9	0.0193	0.0123	–0.0509***	–0.0583***	–0.0287***
	(0.0193)	(0.0101)	(0.00723)	(0.0165)	(0.00992)
10	0.0578**	0.0364***	–0.039***	–0.0599***	–0.0131
	(0.0239)	(0.0125)	(0.00906)	(0.0203)	(0.0124)
11	0.026	0.0451**	–0.0371***	0.00774	0.00393
	(0.0327)	(0.0179)	(0.0127)	(0.0303)	(0.0174)

NOTE: Estimates are weighted averages of cell-level treatment effect estimates, weighted by the share of veterans. Standard errors in parentheses are equal to the square root of the variance of the weighted average of cell-level treatment effect estimates. * = statistically significant at the 5-percent level. ** = statistically significant at the 1-percent level. *** = statistically significant at the 0.1-percent level.

Table A.16

Effects of Military Enlistment on College Degree Attainment, by Military Occupational Specialty and Years Since Military Application, 1991–1994 Application Cohorts: Combat Arms, Electronic Repair, Communications or Intelligence, and Health Care

Years Since Application	Combat Arms	Electronic Repair	Communication or Intelligence	Health Care
2	–0.00132***		–0.000476***	–0.000203
	(0.000282)		(0.000165)	(0.000164)
3	–0.00385***	–0.0023***	–0.00202***	–0.000867*
	(0.000412)	(0.000308)	(0.000295)	(0.00051)
4	–0.00915***	–0.00545***	–0.00576***	–0.00297***
	(0.000617)	(0.000524)	(0.000518)	(0.000996)
5	–0.0183***	–0.0113***	–0.012***	–0.00671***
	(0.000894)	(0.000918)	(0.000891)	(0.00172)
6	–0.0264***	–0.0182***	–0.02***	–0.00563*
	(0.00126)	(0.00152)	(0.00144)	(0.00336)
7	–0.0275***	–0.0224***	–0.0232***	–0.00238
	(0.00185)	(0.00255)	(0.00264)	(0.00516)
8	–0.0245***	–0.0241***	–0.0182***	0.0107
	(0.00244)	(0.00352)	(0.00404)	(0.00735)
9	–0.017***	–0.0204***	–0.0104**	0.0252***
	(0.00293)	(0.00448)	(0.00518)	(0.00882)
10	–0.00856**	–0.017***	–0.00138	0.0465***
	(0.00333)	(0.00518)	(0.00609)	(0.0103)
11	–0.00263	–0.0131**	0.00846	0.0565***
	(0.00362)	(0.00573)	(0.00679)	(0.011)
12	0.00302	–0.00854	0.0134*	0.0715***
	(0.00385)	(0.00619)	(0.00724)	(0.0118)
13	0.00697*	–0.0022	0.0225***	0.077***
	(0.00404)	(0.00663)	(0.0078)	(0.0123)
14	0.0113***	–0.000555	0.0294***	0.0869***
	(0.0042)	(0.00689)	(0.00816)	(0.0127)
15	0.0155***	0.00267	0.037***	0.0936***
	(0.00433)	(0.00716)	(0.00851)	(0.013)
16	0.0164***	0.00815	0.0379***	0.0895***
	(0.00506)	(0.00828)	(0.00963)	(0.0143)
17	0.0227***	0.00629	0.0499***	0.0862***
	(0.00627)	(0.0103)	(0.0121)	(0.0159)
18	0.0246***	0.00586	0.0625***	0.0951***
	(0.009)	(0.0145)	(0.0169)	(0.0216)

Years Since Application	Combat Arms	Electronic Repair	Communication or Intelligence	Health Care

NOTE: Estimates are weighted averages of cell-level treatment effect estimates, weighted by the share of veterans. Standard errors in parentheses are equal to the square root of the variance of the weighted average of cell-level treatment effect estimates. * = statistically significant at the 5-percent level. ** = statistically significant at the 1-percent level. *** = statistically significant at the 0.1-percent level.

Table A.17

Effects of Military Enlistment on College Degree Attainment, by Military Occupational Specialty and Years of Service Since Application, 1991–1994 Application Cohorts: Other Technical or Allied, Functional or Administrative, Mechanical Repairs, Craftsworkers, and Service or Supply

Years Since Application	Other Technical or Allied	Functional or Administrative	Mechanical Repairs	Craftsworkers	Service or Supply
2		–0.000354**	–0.000595***		
		(0.000141)	(0.000197)		
3	–0.00212***	–0.0019***	–0.00261***	–0.00202***	–0.00226***
	(0.000283)	(0.000296)	(0.000312)	(0.000298)	(0.000298)
4	–0.00607***	–0.00512***	–0.00725***	–0.00559***	–0.00613***
	(0.000454)	(0.000559)	(0.00051)	(0.000616)	(0.000501)
5	–0.0129***	–0.0116***	–0.0142***	–0.0112***	–0.011***
	(0.00105)	(0.000939)	(0.000807)	(0.00142)	(0.00104)
6	–0.017***	–0.0202***	–0.0238***	–0.019***	–0.0172***
	(0.00298)	(0.00144)	(0.00122)	(0.00238)	(0.00166)
7	–0.0239***	–0.0252***	–0.0285***	–0.0273***	–0.0203***
	(0.0047)	(0.00246)	(0.00206)	(0.00373)	(0.00277)
8	–0.0213***	–0.0179***	–0.0273***	–0.0292***	–0.0215***
	(0.00704)	(0.00399)	(0.00302)	(0.00574)	(0.00375)
9	–0.0136	–0.0104**	–0.0234***	–0.0255***	–0.0184***
	(0.00946)	(0.00504)	(0.00379)	(0.00759)	(0.00461)
10	0.00825	–0.00266	–0.019***	–0.0228***	–0.0131**
	(0.0122)	(0.00585)	(0.0044)	(0.00872)	(0.00539)
11	0.0193	0.0045	–0.017***	–0.0216**	–0.00331
	(0.0136)	(0.00647)	(0.0048)	(0.00958)	(0.0061)
12	0.0227	0.0128*	–0.0125**	–0.0193*	0.00222
	(0.0143)	(0.00702)	(0.00517)	(0.0102)	(0.00652)
13	0.0259*	0.0189**	–0.00496	–0.0179*	0.0106
	(0.015)	(0.0074)	(0.0056)	(0.0108)	(0.00699)
14	0.033**	0.0257***	0.00056	–0.015	0.0159**
	(0.0158)	(0.00774)	(0.0059)	(0.0114)	(0.00733)
15	0.043***	0.0276***	0.00552	–0.0104	0.0179**
	(0.0166)	(0.00794)	(0.00614)	(0.012)	(0.00752)

Years Since Application	Other Technical or Allied	Functional or Administrative	Mechanical Repairs	Craftsworkers	Service or Supply
16	0.0454**	0.0325***	0.00575	–0.00378	0.0232***
	(0.0189)	(0.00911)	(0.00708)	(0.0139)	(0.00884)
17	0.0737***	0.0376***	0.00884	–0.00372	0.0225**
	(0.0244)	(0.0112)	(0.00872)	(0.017)	(0.0109)
18	0.133***	0.0386**	0.0075	0.000897	0.0153
	(0.0389)	(0.0167)	(0.0124)	(0.0243)	(0.0148)

NOTE: Estimates are weighted averages of cell-level treatment effect estimates, weighted by the share of veterans. Standard errors in parentheses are equal to the square root of the variance of the weighted average of cell-level treatment effect estimates. * = statistically significant at the 5-percent level. ** = statistically significant at the 1-percent level. *** = statistically significant at the 0.1-percent level.

Table A.18
Percentage Earnings Responses to 1-Percent Unemployment Shock at End of First Contract, Men, All

Years to End of Contract	Civilian	Military	Total
–1	–11.263**	0.073	–0.460**
	(3.486)	(0.218)	(0.099)
0	–10.017**	2.544**	0.027
	(1.434)	(0.292)	(0.199)
1	–6.689**	2.771**	–1.142**
	(0.906)	(0.293)	(0.259)
2	–5.973**	2.564**	–1.503**
	(0.941)	(0.349)	(0.334)
3	–4.672**	2.397**	–1.302**
	(1.052)	(0.439)	(0.408)
4	–3.803**	2.503**	–1.097*
	(0.940)	(0.452)	(0.427)
5	–3.112**	2.764**	–0.832*
	(0.811)	(0.518)	(0.379)
6	–2.785**	2.832**	–0.784**
	(0.632)	(0.534)	(0.303)
7	–2.077**	2.224**	–0.614*
	(0.500)	(0.618)	(0.272)
8	–1.546**	1.814**	–0.465
	(0.484)	(0.545)	(0.294)
9	–0.761*	1.334**	–0.113
	(0.386)	(0.517)	(0.232)
10	–0.202	0.557	0.027
	(0.554)	(0.760)	(0.338)
11	–0.206	–0.204	–0.204
	(0.612)	(0.799)	(0.345)

NOTE: Estimates are weighted averages of cell-level treatment effect estimates, weighted by the share of veterans. Standard errors in parentheses are equal to the square root of the variance of the weighted average of cell-level treatment effect estimates. * = statistical significance at the 5-percent level. ** = statistical significance at the 1-percent level.

Table A.19
Percentage Earnings Responses to 1-Percent Unemployment Shock at End of First Contract, Women, All

Years to End of Contract	Civilian	Military	Total
–1	–5.475	0.403	0.18
	(5.481)	(0.256)	(0.198)
0	–6.953**	1.901**	0.739**
	(2.218)	(0.410)	(0.271)
1	–4.010**	2.024**	0.346
	(1.301)	(0.456)	(0.359)
2	–3.543**	1.271*	–0.358
	(1.278)	(0.632)	(0.497)
3	–2.066	0.911	–0.254
	(1.229)	(0.632)	(0.500)
4	–1.7	1.297*	–0.066
	(1.084)	(0.652)	(0.454)
5	–1.363	1.453	0.041
	(1.103)	(0.850)	(0.576)
6	–2.436*	2.457*	–0.275
	(1.180)	(1.052)	(0.615)
7	–1.681	1.854	–0.215
	(0.925)	(1.156)	(0.497)
8	–1.371	0.621	–0.564
	(1.134)	(1.146)	(0.587)
9	–0.044	1.279	0.434
	(0.963)	(1.529)	(0.611)
10	–0.857	1.608	–0.077
	(1.225)	(2.310)	(0.990)
11	0.012	–2.055	–0.41
	(1.334)	(2.616)	(1.112)

NOTE: Estimates are weighted averages of cell-level treatment effect estimates, weighted by the share of veterans. Standard errors in parentheses are equal to the square root of the variance of the weighted average of cell-level treatment effect estimates. * = statistical significance at the 5-percent level. ** = statistical significance at the 1-percent level.

Table A.20
Percentage Earnings Responses to 1-Percent Unemployment Shock at End of First Contract, Men, Army

Years to End of Contract	Civilian	Military	Total
–1	–9.190**	0.699**	0.088
	(3.785)	(0.233)	(0.110)
0	–6.904**	1.264**	–0.358*
	(1.945)	(0.467)	(0.157)
1	–5.133**	1.621**	–1.113**
	(1.288)	(0.596)	(0.270)
2	–4.713**	1.549**	–1.338**
	(1.280)	(0.552)	(0.393)
3	–3.725**	1.585*	–1.148*
	(1.351)	(0.645)	(0.499)
4	–3.117**	1.813*	–0.985*
	(1.188)	(0.704)	(0.495)
5	–2.483**	2.488**	–0.552
	(0.929)	(0.749)	(0.422)
6	–2.222**	2.411**	–0.558
	(0.780)	(0.777)	(0.371)
7	–1.763**	2.098*	–0.429
	(0.591)	(0.886)	(0.332)
8	–1.191*	1.755	–0.265
	(0.596)	(0.920)	(0.368)
9	–0.817	1.234	–0.195
	(0.577)	(0.763)	(0.354)
10	–0.36	1.81	0.295
	(0.789)	(1.143)	(0.490)
11	–0.165	–0.14	–0.161
	(0.888)	(1.403)	(0.551)

NOTE: Estimates are weighted averages of cell-level treatment effect estimates, weighted by the share of veterans. Standard errors in parentheses are equal to the square root of the variance of the weighted average of cell-level treatment effect estimates. * = statistical significance at the 5-percent level. ** = statistical significance at the 1-percent level.

Table A.21
Percentage Earnings Responses to 1-Percent Unemployment Shock at End of First Contract, Men, Navy

Years to End of Contract	Civilian	Military	Total
–1	–11.632**	–0.384	–0.761**
	(3.868)	(0.273)	(0.200)
0	–10.770**	1.768**	–0.158
	(1.368)	(0.325)	(0.190)
1	–8.043**	3.008**	–0.753**
	(0.904)	(0.363)	(0.255)
2	–7.024**	2.923**	–1.349**
	(0.855)	(0.541)	(0.300)
3	–5.453**	2.607**	–1.289**
	(0.931)	(0.668)	(0.361)
4	–4.615**	2.614**	–1.161**
	(0.895)	(0.673)	(0.387)
5	–3.389**	2.806**	–0.696
	(0.855)	(0.740)	(0.371)
6	–3.145**	2.624**	–0.861**
	(0.726)	(0.905)	(0.326)
7	–1.764**	1.768	–0.449
	(0.648)	(0.925)	(0.377)
8	–1.162	0.866	–0.416
	(0.654)	(0.870)	(0.382)
9	–0.168	–0.009	–0.069
	(0.855)	(0.881)	(0.455)
10	–0.756	–1.274	–0.87
	(0.923)	(1.256)	(0.519)
11	–0.15	–1.311	–0.292
	(1.101)	(1.219)	(0.531)

NOTE: Estimates are weighted averages of cell-level treatment effect estimates, weighted by the share of veterans. Standard errors in parentheses are equal to the square root of the variance of the weighted average of cell-level treatment effect estimates. * = statistical significance at the 5-percent level. ** = statistical significance at the 1-percent level.

Table A.22
Percentage Earnings Responses to 1-Percent Unemployment Shock at End of First Contract, Men, Marine Corps

Years to End of Contract	Civilian	Military	Total
–1	–9.984*	–0.747*	–1.034**
	(3.998)	(0.300)	(0.187)
0	–10.734**	4.469**	0.341
	(1.438)	(0.609)	(0.541)
1	–5.577**	3.174**	–2.112**
	(0.614)	(1.015)	(0.489)
2	–4.897**	2.400**	–2.238**
	(0.790)	(0.914)	(0.500)
3	–4.062**	3.188**	–1.597*
	(1.051)	(1.074)	(0.658)
4	–3.034**	3.185**	–1.31
	(0.898)	(1.203)	(0.688)
5	–2.722**	2.255	–1.508*
	(0.876)	(1.253)	(0.686)
6	–2.514**	3.097*	–1.229*
	(0.819)	(1.469)	(0.558)
7	–2.178**	2.001	–1.243*
	(0.775)	(1.814)	(0.499)
8	–1.899**	1.315	–1.219*
	(0.676)	(1.554)	(0.522)
9	–0.903	1.377	–0.439
	(0.524)	(1.840)	(0.455)
10	0.021	2.071	0.386
	(0.751)	(2.770)	(0.644)
11	0.061	–1.91	–0.025
	(0.799)	(3.134)	(0.71)

NOTE: Estimates are weighted averages of cell-level treatment effect estimates, weighted by the share of veterans. Standard errors in parentheses are equal to the square root of the variance of the weighted average of cell-level treatment effect estimates. * = statistical significance at the 5-percent level. ** = statistical significance at the 1-percent level.

Table A.23
Percentage Earnings Responses to 1-Percent Unemployment Shock at End of First Contract, Men, Air Force

Years to End of Contract	Civilian	Military	Total
–1	–9.396**	0.046	–0.440**
	(2.985)	(0.224)	(0.155)
0	–11.341**	3.114**	0.354
	(1.080)	(0.276)	(0.227)
1	–8.147**	3.484**	–0.522
	(0.824)	(0.305)	(0.306)
2	–7.277**	3.181**	–1.029**
	(0.785)	(0.387)	(0.328)
3	–5.564**	2.512**	–1.118**
	(0.927)	(0.574)	(0.405)
4	–4.723**	2.861**	–0.855*
	(0.907)	(0.719)	(0.420)
5	–4.387**	3.545**	–0.676
	(0.944)	(0.897)	(0.479)
6	–3.766**	4.002**	–0.461
	(0.811)	(1.039)	(0.427)
7	–3.095**	3.667**	–0.354
	(0.788)	(1.169)	(0.512)
8	–2.448**	3.889**	0.044
	(0.873)	(1.268)	(0.594)
9	–1.342	4.009**	0.676
	(0.787)	(1.420)	(0.545)
10	0.979	1.39	1.109
	(0.832)	(1.705)	(0.631)
11	0.16	1.812	0.405
	(0.929)	(1.543)	(0.690)

NOTE: Estimates are weighted averages of cell-level treatment effect estimates, weighted by the share of veterans. Standard errors in parentheses are equal to the square root of the variance of the weighted average of cell-level treatment effect estimates. * = statistical significance at the 5-percent level. ** = statistical significance at the 1-percent level.

Table A.24
Percentage Earnings Responses to 1-Percent Unemployment Shock at End of First Contract, Men, No Reenlistment (Corrected for Selection)

Years to End of Contract	Civilian	Military	Total
–1	–4.1	–0.368	–0.589**
	(2.883)	(0.275)	(0.110)
0	–7.311**	3.227**	–0.906**
	(1.152)	(0.677)	(0.323)
1	–4.461**	2.363	–3.632**
	(0.656)	(1.757)	(0.471)
2	–3.878**	–0.003	–3.550**
	(0.693)	(2.495)	(0.571)
3	–2.847**	1.246	–2.469**
	(0.837)	(2.217)	(0.721)
4	–2.358**	1.884	–1.946**
	(0.807)	(1.956)	(0.714)
5	–2.011**	3.128	–1.524*
	(0.727)	(1.908)	(0.643)
6	–1.656**	2.551	–1.253**
	(0.572)	(1.964)	(0.480)
7	–1.111**	–0.17	–1.022**
	(0.419)	(2.028)	(0.337)
8	–0.832*	–0.83	–0.848**
	(0.356)	(2.027)	(0.326)
9	–0.238	–0.636	–0.281
	(0.295)	(1.708)	(0.256)
10	0.577	–5.047**	0.068
	(0.559)	(1.808)	(0.459)
11	0.035	–1.81	–0.09
	(0.589)	(1.922)	(0.491)

NOTE: Estimates are weighted averages of cell-level treatment effect estimates, weighted by the share of veterans. Standard errors in parentheses are equal to the square root of the variance of the weighted average of cell-level treatment effect estimates. * = statistical significance at the 5-percent level. ** = statistical significance at the 1-percent level.

Table A.25 presents estimates of the relationship between PaYS take-up and earnings. The first column shows total earnings, while the second and third columns break earnings into civilian earnings and military earnings. The results indicate somewhat lower earnings among PaYS participants in years 3, 4, and 6. These results are driven by differences in civilian earnings.

Table A.25

Effects of Army Partnership for Youth Success Program on Earnings, by Earnings Type and Year Since Accession, Army Enlistees Signing Four-Year Contracts and Separating After One Term

Years Since Application	SSA Earnings	Civilian Earnings	Military Earnings
1	–448	–201	–706
	(261)	(324)	(552)
2	–538*	–344	–426
	(295)	(359)	(545)
3	–983**	–796**	–336
	(365)	(375)	(571)
4	–1,168**	–1,035**	–217
	(488)	(500)	(364)
5	–1,085	–1,044*	–25
	(691)	(613)	(528)
6	–1,831**	–1,606**	–92
	(878)	(832)	(645)
7	–940	–557	–595
	(1,326)	(1,318)	(650)

NOTE: Estimates are weighted averages of cell-level treatment effect estimates, weighted by the share of veterans. Standard errors in parentheses are equal to the square root of the variance of the weighted average of cell-level treatment effect estimates. * = statistical significance at the 5-percent level. ** = statistical significance at the 1-percent level.

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